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REFLECTIONS ON THE PRACTICAL APPLICATION OF ECONOMICS[1]

1. It is the privilege of the President of the Society to treat himself as an elder statesman and to make his address on some very general subject. I am the more led to follow this practice because of the nature of my own work in recent years. I must confess to feeling from time to time, as I read the pages of our own JOURNAL, that there is a considerable gulf between what I do and at any rate the more mathematical articles which seem to fill an increasing amount of our space. Doubtless there is a personal element in this. But I have been led to wonder whether there may not also be some features in the present conduct of our discipline which require re-emphasis. I am going to speak, then, on those very broad topics which involve the nature and usefulness of economics, about which we all think from time to time. And if there is any interest in a collection of things which have been said already, it must come from my own arrangement of pieces, most of which are already familiar.

2. The word "practical," of course, implies doing something, and those who make use of economics for purposes other than the pleasure of intellectual exercise obviously want to discuss things that are occurring or are likely to occur. This requires one to be able to take a view about what is going to happen, and if one does not like the prospect, to be able also to take a view as to whether anything can be done about it or whether one must put up with the inevitable. It may also be the case that where there is the possibility of action, one is not very clear as to what one ought to want and therefore to do. These are perfectly general formulations, applicable as well to other fields as to economics.

3. I shall confine myself mainly to economics in relation to public policy, and within that to the more general issues; and to speak also of Western economies and mainly of the United Kingdom. As Mr. Seers[2] has reminded

[1] Presidential address delivered to the annual meeting of the Royal Economic Society on July 9, 1959. I am much indebted to Mr. Bryan Hopkin and to Mr. J. Downie of the Economic Section of the Treasury for comments and suggestions. I was also helped by Professor Tress' lecture on "The Contribution of Economic Theory to Prognostication" (published in *Economica*) and by several papers by Professor Ely Devons (one of which was published in *Lloyds Bank Review*, July 1959).

[2] Dudley Seers, "An Approach to the Short-period Analysis of Primary Producing Economies," *Oxford Economic Papers*, February 1959.

us in a recent number of *Oxford Economic Papers*, a good deal of misunderstanding can arise in applying the lessons of one economy to another. My concentration on public policy is partly an occupational bias. But it is also because I suspect that one of the main uses of professional economists to business-men is in presenting them with the informed and coherent view of general economic developments which is also one of the chief preoccupations of the Government economist. I suspect that, in the determination of policy for a business, the irreducible area within which intuition is the best guide is rather wide. Even if computers enable us to act on what comes out of them, we will never get away from what we are to put in, and in business the intuitive input must be high.

4. I will first deal briefly with the third of the questions I posed a moment ago; the question of what you ought to want.

5. Many would say that the question of what we want or should want is not within the professional competence of economists. Within limits, I agree. In the practical world there is, strictly speaking, no such thing as an economic action any more than there is any other kind of action—there are only actions, with all the attendant circumstances. Thus if the economist is asked about what ought to be done, it must be either because he is being asked as a whole man rather than as an economist, or else the question implies agreement about the end, so that it is really about the means.

6. But it is an illusion to imagine that the dispassionate economist beloved of those who labour the distinction between ends and means, between normative and positivistic economics, can live in the practical world. It is an illusion which arises from forgetting to release from *ceteris paribus* at the end the multiplicity of interconnected objectives which exist in practice, but which are, quite rightly, assumed away at the beginning of the reasoning.

7. Any action has repercussions which are in principle unlimited. It is important in practice to hold to the strong shield of the doctrine of *de minimis*, lest we should end up by denying the possibility of knowledge, as some general equilibrium theorists have come very near to doing. Nevertheless, it is true that in practice what we want is always a *total* situation which we prefer to any other of the alternative total situations open.

8. And however much, in any particular instance of choice, our attention has to be concentrated on a particular objective, yet we shall be unhappy in the outcome if we neglect all the other objectives, and the fact that they are connected, in the sense that progress towards one will have implications for the others.

9. Put in this very general form, this is a commonplace. Everyone knows that you cannot have your cake and your penny too. But at the level of practical choice the inter-relations between choices are very complex. It is one of the merits of an economic training (indeed of scientific training generally, and social scientific training for our particular case) that it teaches us to consider always the inter-relationships of the particulars without

losing sight of the general. Thus, if I may be somewhat immodest on our behalf, the economist may sometimes know what his client " really " wants better than he does himself.

10. This doctrine that we can, so to speak, put ourselves into other people's minds is a dangerous one. It would be salutary for the practical economist to hang on his wall the forcible statement of Adam Smith [1] on this subject. Nevertheless, the idea that other people can express unaided what it is now fashionable to call their social-preference functions is also dangerous. The number of related objectives and the complexity of their inter-connections is often too great for the client to be able to formulate his objectives unaided. For example, it is only now ceasing to be a paradox to the layman that there are cases where you *can* have your cake and your penny too, *e.g.*, that you can increase consumption by increasing investment if you have unused resources. The idea that the best way to cure inflation may sometimes be to raise prices is still very shocking to the layman. And so on.

11. I suggest therefore that the economist can be a great help as an elucidator. There are, of course, limits to his approach. The economist, who is trained to measure quantities either in money or in terms of real costs compared by the possibility of substitution at the margin, is always in danger of minimising or neglecting the forces which cannot easily or accurately be expressed in these terms. The practical man, since he is dealing with reality from the beginning, is very unlikely to make this mistake. Take, for example, the dislike which is so prevalent among human beings of being subjected to the forces of economic change, especially when the short-term effects are financially adverse. This is at the root of the idea of what is " fair and reasonable " which is just as prevalent to-day as it has been at other periods of history. This dislike is very real to the politician, who has to evaluate it in the sense that he may have to take a decision as to whether to acquiesce in or promote or mitigate or prevent a change. Any help which he could get from the economist would clearly be extremely useful.

12. What this suggests to me is that the economist who is interested in welfare questions can make his characteristic or professional contribution, not as I think many economists still tend to do, by condemning various practices out of hand, but by trying to identify all the forces at work and trying to measure the cost of as many as possible in money terms. As I have said, this is unlikely in itself to yield a clear-cut answer. Thus if the objective is to keep a given number of people in a given area, to move them elsewhere is certainly not an exact alternative. But it would certainly be useful

[1] *Wealth of Nations*, Book IV, Chapter 2, " The statesman who should attempt to direct private people in what manner they ought to employ their capitals . . . would . . . assume an authority . . . which would nowhere be so dangerous as in the hands of a man who had folly and presumption enough to fancy himself fit to exercise it."

It is not so much that the statesman, or Civil Servant, would not be capable of making better decisions than many business-men, as that he has not time or information in the detail needed.

to know what this would cost, and also what it costs to keep them where they are. Another practical problem of this kind is presented on a large scale by the use of land, not only for agriculture but also in general. There is plenty of scope for economic analysis as well as for empirical research, since the problems of determining the costs involved—of getting the question into a shape where one can set about its measurement—are usually quite severe exercises in economic thinking. And anyone who is engaged in this kind of work can be fairly sure that he is doing something well worth while in the practical sense.

13. I come now to my central theme, namely to review the help which economics does and could give in predicting what is going to happen and of saying how something different can be made to happen. I need scarcely emphasise that the two problems are essentially the same. In one case the problem is to forecast the unrolling of events if certain strategic variables are left to work themselves out undisturbed. In the other case the task is to forecast how events will unroll if these strategic variables are changed, and how they may be changed.

14. Nor, in emphasising prediction, am I seeking to make a distinction between this and understanding. Indeed, if I understand them rightly, philosophers to-day maintain that all understanding and knowledge contain a predictive element. What we have to do, in order to understand and predict, is to impose some kind of structure on the undifferentiated flux of events. This involves two activities: the imposition of concepts by which we can classify the flow of events into a manageable number of classes, and the formulation of hypotheses about the empirical inter-relationships between these classes.

15. I must emphasise this distinction between concepts and hypotheses because, though quite elementary, it is also fundamental, and frequently neglected in practice. Hypotheses can be true or false, but concepts can only be useful or useless. The characteristics of a useful set of concepts are: first, a minimum condition, that they should be internally consistent; second, that they should be capable of having quantitative statements made about them, cardinal or ordinal; thirdly, that it should be possible in practice to establish functional relationships between the concepts.

16. Let me take a very simple example; the simplest possible version of the Keynesian model in a closed system. The " value added " by the factors of production engaged in a business enterprise is defined as the excess of receipts from current sales over current expenditure by the enterprise on goods and services purchased from other enterprises. The sum of value added for all enterprises (broadly defined) is the national income, Y. We then define C as current expenditure by non-business enterprises; and I as $Y - C$. We so establish the fundamental definitional relation $Y \equiv C + I$.

17. This expression is capable of mathematical manipulation in all sorts

of ways. In particular, without additional assumptions or definitions, we can write

$$\Delta Y \equiv \Delta I \frac{1}{1 - \frac{\Delta C}{\Delta Y}}$$

18. But, however much we manipulate the expression, it can tell us nothing about the nature or behaviour of reality, because the initial definitions contain no statements about the nature of reality.

19. But if we formulate the *hypothesis* $\frac{\Delta C}{\Delta Y}$ is constant we immediately have a most powerful instrument for predicting the behaviour of reality. If we go further, and postulate a second functional relationship between I and *previous* values of Y, we then have a member of the well-known, and now quite extensive, family of trend and cycle models.

20. I have quoted this example, not because I believe that these particular functional relationships are valid. Experience has shown that in fact, we do not know enough about the multiplier or the accelerator relations to enable us to use them in any simple way in forecasting—even when the multiplier is formulated in complex matrix terms. I have used it, because the system of which it is the essence gave birth to one of the most fruitful developments in practical economics during the last quarter of a century—namely, national accounting.

21. I use this term in a wide sense, to mean not only the net accounts which appear in our Blue Book and in similar publications in other countries but also the various disaggregations of the net accounts, which belong to the same family—*e.g.*, input/output tables or analyses of money flows.

22. I have already described the specifications which a set of concepts must satisfy if they are to be useful or fruitful; namely, consistency, measurability and that it should be possible to establish empirical relationships between them. How far does the conceptual structure of national accounting meet these requirements?

23. A great deal of work has gone into establishing the *consistency* of the definitions which are employed. Argument in detail still continues—I see that the proper treatment of import duties in the measurement of Gross Domestic Product is even now being hotly debated in the JOURNAL. But we now have a body of theoretical wisdom on the subject which rivals in thoroughness that of the medieval church. We have a structure of great flexibility, capable of disaggregation, in principle, right down to the individual household or firm.

24. As regards the measurability of the concepts, there is no longer much reason to complain. Sir Harry Campion, in his Presidential Address to the Royal Statistical Society this year, reviewed the present position on official statistics in this country. Thanks largely to his own efforts, and the

support of the statistical divisions in the Government Departments concerned, we can at least claim that we have a fairly full and accurate picture of where we were in the not too distant past, and work is going on all the time to make this more complete and nearer to the present. The user of statistics is, of course, never likely to be satisfied with what he has got; and there is a constant, though on the whole an amicable struggle, between the economist who wants everything now, the statistician who wants everything right and the raw material, the unfortunate supplier of the statistics who has to fill in the forms. We should all be grateful to the Federation of British Industries for the help which they give us: their members in turn, themselves engaged in forecasting, are, of course, helped by any improvements both in statistics and in their interpretation and application. We have at any rate something better than the " last year's *Bradshaw* " to which Mr. Macmillan referred when he was Chancellor.

25. But, as I have said, one set of concepts does not of itself say anything about what is going to happen. The only relations we can derive from a set of definitions are those logically implied by the definitions. Mathematical manipulation can suggest new and illuminating ways of bringing out these implications, but it cannot add any more content. We gain insight into reality and enable ourselves to pass from statistical material to something that will enable us to say with confidence what that material will be in the future, only by establishing empirically the relationship between our concepts.

26. It is when we come to consider the body of usable empirical functional relationships between the national-income categories that the gulf between theory and practice to which I referred at the beginning of my lecture yawns so wide. In his lecture on " The Contribution of Economic Theory to Economic Prognostication " Professor Tress has reviewed the field extensively and found it wanting. I have nothing to add in general except to say that my own experience in practical forecasting bears out his conclusion, that our models are very ill-clad with relationships.

27. As regards our own short-term forecasting, we do have some explicit empirical equations—tax functions naturally, price equations which do not simply contain a constant margin and so on. But, by and large, the models we use are extremely simple ones, and we find that for short-run forecasting it is much more reliable to collect forecasts of the components, where these are available, from people who will actually make the decisions or undergo the experiences, and to subject the other quantities to a simple process of extrapolation, modified as best one can by general knowledge, judgment and experience, than to use any more complex functional relationships. The process is well illustrated in the United Kingdom by the annual *Economic Surveys*, or the appraisals by the Chancellor of the Exchequer in annual Budget speeches. In these also a clear distinction can be seen between those quantities which can be reasonably got at in this way and those which

cannot, of which the most obvious example is stocks. "The last and most uncertain element in home demand is expenditure on industrial and commercial stocks." Our forecasts of export also, though based on detailed study, naturally involve some conjecture.

28. When it comes to long-term forecasts, which have become features of many countries, both those which are very rigidly controlled and those engaged on development, there are some theoretical reasons for expecting things to be better. The number of terms which can be handled in any equation is fairly strictly limited in practice. Hence, however cleverly our concepts are framed, we are bound to leave out some factors. But if we have chosen our concepts wisely, the excluded factors will be truly random, and so can safely be ignored in the long run, since their influence will be zero. But, in practice, I doubt whether economic science has yet contributed to long-term forecasting more than a few very general ideas, though these are important ones.

29. It may be thought that, in adopting this critical tone, I am finding fault with the nature of reality rather than with the practice of economics—a sin which it is peculiarly easy to commit.

30. So far as the long term is concerned, I think there is much truth in this. When we are dealing with the growth of output in physical terms, what is needed is essentially technical rather than economic information. An under-developed country needs to know how much steel a particular plant will turn out, how long it will take to train the labour and how efficiently it will operate; what sort of land it has, what sort of methods of cultivation could be applied to it, what are the social and technical problems involved in getting a change in methods; and so on. It is a very long and laborious process, first, to make a plan and, second, to put it into operation, and it is quite certain that at every stage things will be found to be going differently from what had been expected, and adjustments will have to be made. Thus the uncertainties inherent in any process of growth are likely to be so great that the price adjustments and the income adjustments which will have to be made as time goes on can be foreseen only as types of problem —or at any rate unless the original plans are constructed with so little relation to past experience as to make it probable that there will be a clear lack of balance between different components.

31. Indeed, I go so far as to wonder, where long-term projections are concerned, whether the main danger is not that under-developed countries will expect too much of economists, rather than too little; so that perhaps we ought actively to be engaged in reducing our image to more proper proportions. In particular, it seems to me that it is more necessary to be aware of the limitations than of the advantages of the capital/output ratio concept to which so much attention is now devoted. Economics was created in developed countries, and there is always a danger in applying its theories to under-developed countries without considering whether this is appro-

priate.[1] Even if we did understand the history of our own development, the initial circumstances of most of the now developed countries were very different from those now confronting the present under-developed ones.

32. As regards short-run forecasting, it may very properly be asked why, if one can ask people what they are going to do, one should hanker after functional relationships which would allow their action to be inferred from other given data. Moreover, I have already suggested that there are ineluctable uncertainties in the short run, for example, the weather, political uncertainties, labour disputes and so on. Indeed, the process of short-term adjustment of the economy, for these and other reasons, is often compared to other types of practical operation, driving a car or steering a ship. There is a general idea of the direction in which it is intended to move and of the conditions which will be encountered. But we do not know enough about the conditions either of the mechanism (the economic model) or the circumstances which will be encountered (the statistical background and the exogenous variables) to dispense with the driver or attendant, as may be done often with stationary machines for long periods. And if the driver has to be there he will do better to look out and adjust himself to the developing situation than to assume that his calculations will see him through.

33. Far be it from me to under-rate the virtues of the empirical approach to policy—the constant adjustment of the rudder. Still less do I want to decry the importance of general knowledge, judgment and experience—intuition. Indeed, if intuition were given the " scientific " name of " non-statistical inference " no one would look down his nose at it. Intuition may indeed be defined as the quality which, pragmatically speaking, sets men above machines.

34. There is much truth in all this. Indeed, since we are dealing with people, the basis of our discipline is obviously more akin to that of medicine than to the exact sciences. But we must not go too far in finding excuses for ourselves, or accept the present situation with complacency.

35. One might almost define scientific progress as the reduction of the area in which we have to rely on intuition or trial and error. The inevitable uncertainties with which we are faced in the short run are perhaps not so great as is often thought. While the weather in some countries produces uncertainties which upset all calculations, in Britain the availability of supplies from all over the world reduces the range of variation to quite small figures. Perhaps the most inconvenient effect is that of the vagaries of the potato crop on the retail price index, itself an important economic quantity. Because of the ratchet operation of wages, it could be argued that fluctuations in potato prices have an inflationary effect, and it would be an interesting study for Mr. Dow and Mr. Dicks-Mireaux to see if there is anything in it.

36. Other short-run uncertainties are mainly on the technical side, and

[1] Cf. Dudley Seers, *op. cit.*

on exogenous shifts in demand, which again are much more difficult for less-diversified economies than our own; and the broad class of changes which could be labelled social and political. Wars and rumours of war, changes in political regimes and so on are things with which mankind has always had to deal so far, and with which it looks as if mankind will continue to have to struggle for some time to come. From the economist's point of view, everyone knows that they are uncertain, no one expects him to be able to predict them, and there is usually little of which he should feel ashamed if he gets them wrong. What at first sight seems to be a large political uncertainty—the incidence of labour disputes—is another field in which the economist as such is no better able to predict than anyone else. But in recent years they have in fact had very little effect on the general economic situation, and looking back at most economic series, it would be difficult to detect them apart from unevennesses in such figures as the monthly Trade Returns.

37. When it comes to asking, rather than predicting, what people are going to do, it is important to remember the limitations on the direct approach. In many cases, " don't know " would be the accurate answer to a question. But even when it is not, people often change their minds in the time elapsing between answer and action, because of changes in general economic circumstances. Some of them, and perhaps some of the " don't knows," could be foreseen by those engaged in wider economic analysis. For example, economists in the United States began quite early to discount the rather gloomy forecasts of private investment intentions which businessmen were making in the later part of last year. As successive samples have been taken, showing a steady upward revision of intentions, the wisdom of this discounting has been justified. In the second place, great care needs to be taken in interpreting people's answers to questions, especially if they have an interest in the action to which their answers will lead. For example, I would not have great faith in the accuracy of answers to questions about what would be the effect of tax changes.

38. But perhaps the most compelling reason why I think we cannot rest at all content with the present position is the fact of the very wide differences of opinion which appear to exist between economists on quite fundamental aspects of policy. Such differences have long been a popular butt for the layman, and it is only fair to say that his impressions are sometimes based on misunderstanding. Nevertheless, there is a disturbing amount of truth in what he says.

39. Let me take only two simple and well-known examples. It is now a major problem of Western countries to find a method of reconciling a high level of employment with price stability. This is a practical problem in the sense that most Governments would very much like to be able to do it, as is quite obvious from reading the continuous official pronouncements on the subject. Economists have been, and to some extent still are, broadly

divided into those who think that prices rise because of excess demand which creates conditions which pull up prices, and those who think that even if there is not excess demand (unless this is defined by the existence of rising prices) prices will still rise because the modern economy offers no obstacle to their rising except in conditions of very severe unemployment. On the first view there ought to be a very close correlation between total demand in relation to resources and the movement of prices, while on the second there is an indeterminate element, and the problem has to be attacked partly by social or institutional changes.

40. The confusion which exists on this subject was well illustrated in a recent number of the *Scottish Journal of Political Economy*,[1] which was entirely devoted to it and where a number of reputable writers put forward very varied views, not only on the causes of price increases but even on the facts which they were trying to explain. If it is any comfort to economists in this country, a recent American symposium on the subject of *Economic Stability and Growth*, which contains no less than 712 pages and over forty contributions, contains in the General Summary at the end, in an article by Professor Ackley, the following passage. He is discussing the two schools of thought to which I have referred, the demand inflation and the wage/cost inflation.

> " Economists have talked so much about these two kinds of inflation in recent years that it seems to me strange that they have not been able to say more about what has been happening. In particular, I would like to know to what extent the inflation in the United States since the war has been demand inflation and to what extent, if any, it has been wage cost inflation. I find very little discussion and no agreement on this question."

41. Another example is in the question of the use of interest rates as a general economic regulator. I remember the symposium published by the Oxford University Institute of Statistics in 1952, soon after the revival of an active Bank Rate policy in the United Kingdom. It seemed to me then that there was a good deal of uncertainty, and this has really been going on among economists for a very long time. The main difficulty from a practical standpoint, in assessing the various theories on the subject, is that one is often left in uncertainty as to the various causal steps which are implied in a particular view, and it is thus impossible to subject them to any sort of empirical check. Indeed, there is still a tendency to speak in magical rather than scientific terms of the use of interest rates and of monetary controls generally. The economy is to be given a potion or dose, and wonderful results will follow. Other examples will occur to everyone familiar with the subject.

42. What I have said so far may perhaps suggest that I think that economics is of little practical value, apart from, so to speak, training the

[1] For June 1958.

drivers or the helmsmen. But this would clearly be going too far. Having been critical, I ought to say something in conclusion about the way in which things might be improved. I do so with some trepidation, and am uneasily conscious that much of what I have to say may seem very obvious. But unless the practical economist is prepared to tell his academic colleagues something of what he wants, he can scarcely claim the right to complain.

43. First, as to the problems to be worked on. It is not for me, or anyone else for that matter, to try to lay down a research programme for the profession. But it is useful to bear in mind that economics began as an attempt to clarify the issues of political dispute; it has been said that its proper function is metapolitical. While I do not suggest that the whole profession should henceforward devote itself to controversy on issues of current policy—my life would be even more difficult if it did—I do suggest that practical preoccupations can be a valuable guide to the direction of research. Logical precision for its own sake is a very beguiling siren—it took centuries before people gave up the attempt to square the circle—and one needs strong protection against this temptation.

44. I must confess that I am often surprised as much by the subjects which are not worked on as by those which are. I speak with diffidence here, for I do not pretend to have more than a very general acquaintance with work in progress. But I have been struck, for example, by the fact that, although price theory has always occupied a large part in the profession's attention, surprisingly little work seems to have been done so far on such questions as the variability of profit margins with the general level of activity, which modern statistics now make it possible to investigate. The multiplier was understandably out of fashion so long as the economy was largely supply-determined, as it was in the earlier post-war years. But, now that we have returned to more normal conditions, I would have expected it to recover its attractions as a subject for research. Indeed, much of the discussion about the probable consequences of the last Budget has turned on what value the multiplier has. The relation between exports and the level or rate of growth of home demand has been another very important practical issue, but again I have come across little academic work which bears on it. Indeed, the whole subject of the balance of payments seems to be suffering from some neglect, particularly in its dynamic aspects.

45. These are only particular examples, intended to illustrate and not to exhaust. I wonder also if we are not in danger of taking our conceptual apparatus too much for granted. The classes into which we divide economic events—be they commodities, classes of expenditure, types of output or of income, etc.—are all aggregates of individuals with differing characteristics. We tend to assume that totals or averages for the classes will summarise all the relevant characteristics of the frequency distributions within them. This is by no means self-evident. For example, to the best of my knowledge, those who have taken part in the discussion of wage drift in recent years

have all assumed implicitly that the differing rates of growth of average wages and average earnings which they are investigating result from differing rates of shift in the two frequency distributions whose characteristics are summarised by these averages, and not from mere changes in the shapes of the distributions. I would expect that the nature of the explanation of wage drift might differ considerably according to whether this assumption is true or not.

46. More generally, there continue to lie unused large banks of potentially valuable statistical data, in the shape of the primary material of which official statistics provide one form of summary. There are obvious practical difficulties about more intensive work on this material, and I do not wish to prejudice in any way the position of the official statisticians. But there seems to me to be here scope for valuable work by concept makers and concept users.

47. As to methods of work, there is first the old and simple rule that, if your facts or theories lead to conclusions which are at variance with your common sense, then you need to look again at your facts or theories. This recommendation may seem so simple as to be almost insulting. But, if I may make a personal confession, I am frequently struck by how much value I get from my academic colleagues in oral discussion compared with what I learn from their writings. Indeed, much of the profession seems to exist in a state of controlled schizophrenia—once paper is on the table, good sense and a sense of proportion seem to fly out of the window.

48. Narrowing so far as possible the limits within which there is disagreement between economists can be done with any prospect of success only by appeals to experience. It follows therefore that the most useful economists, from a practical point of view, are those who check their results against what has happened already.

49. This is a laborious and expensive task which is quickly brought up by limits of time and money. It seems to me, however, that we often make things more difficult for ourselves than we need to do, simply because we do not work in a standard form, which would at least make it possible to compare different pieces of work in the same field. Thus even if the resources are not available for the empirical research which would be needed to test any particular piece of theoretical work, much could be done by defining the terms in ways which would have a precise meaning to economic statisticians.

50. It hardly needs to be said that it is also an essential requisite that if forces are thought to be at work, the person writing about them should explain exactly how the changes which he is predicting would take place. For otherwise it becomes impossible for anyone else working on the same subject to speak the same language. To elaborate a little on a simple example to which I have already referred, if it is thought that monetary policy will produce a change, one must define the sectors which it is ex-

pected will be affected and how the processes will work. If monetary restrictions affect borrowers, will they be as Sir Ralph Hawtrey has argued for many years, in the first instance the holders of inventories? or will they include those who are borrowing for fixed investment? And if the monetary restrictions are supposed in turn to affect the movement of prices, what is the route through which changes in investment will affect the price level?

51. The explicit formulation of hypotheses in this way would help to guard against what I regard as a besetting sin among economists; that of enunciating purely definitional relationships when they purport to be making statements about reality. I have referred already to the difficulty of resolving disputes in the field of monetary policy arising from the truncated, not to say mystical form, in which theories are enunciated. The dangers can be illustrated from the report made by the staff of the International Monetary Fund last year on International Reserves and Liquidity. This is in many respects a most admirable document. But it is constantly on the edge of suggesting that because, *ex post*, changes in reserves are identically equal to the excess of saving over home and overseas investment, all that a country needs to do in order to build up its reserves is to raise slightly its rate of saving in relation to the rate of investment.

52. This sin is so pervasive that I almost wish that it were within our power to mark out in some way any economist who failed to use consistently the identity symbol when writing a definitional equation and to confine the use of the equals symbol to empirical relationships or hypotheses.

53. Proper formulation will also help with the problem of communication between different kinds of economists. It is becoming more and more difficult for the practical economist to learn from his academic colleagues or, I suspect, for any but the most industrious in the academic field to keep up with developments in other branches than his own. This is partly because of the gap between the mathematical and the non-mathematical economist. But it also results from the bad habit of economists of inventing (different) individual definitions for words in common use. In addition, there is a very real need for the occasional short and easily comprehensible review of the state of knowledge, including empirical knowledge, of the particular branches of the subject. The task of preparing them may seem to many academic economists to be so pedestrian as to be beneath them. I remember, however, that, before the War, the annual review of economic theory in *Econometrica* was much more than a mere summary; the stimulus of compiling it often produced new ideas. Since the War we have had two extremely valuable surveys of contemporary economics produced in America. Neither the time nor the expertise of the practical economist is unlimited, and it would certainly help him if he knew where he was, both in the progress of the subject and in its terminology.

54. All this sounds so extremely elementary that it may be asked if it is worth saying at all. And yet we come back to the large measures of disagree-

ment among economists about the objects and methods of practical policy, which can only arise either from the inability to get any facts with which to check the theories, in which case the theories cannot be said to have any practical application; or from the inability of economists to put their results in a form in which they can be checked empirically; or finally from the lack of resources to make the check.

55. There have always been economists who have sought to combine theory with practice. I think that their number is increasing. And in recent years we have seen the development of such bodies as the Institute of Statistics in Oxford, the Department of Applied Economics in Cambridge and the National Institute of Economic and Social Research in London. Similar bodies exist, here and in other countries—their names will be familiar to you all. If economics is to make further progress as a body of knowledge which can be applied to practical affairs it is to the collaboration of economists with bodies such as these that we must look.

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THE PROFITABILITY OF INVESTMENT

I DO not propose in the course of these lectures [1] to advance any new model of economic growth—a model in which profitability of investments has been specifically elaborated. Unquestionably we need new and better theoretical formulations of the problems that I shall examine—I am quite aware of this—but in the international division of labour between economists I find that my own comparative costs for the skilful art of model-building are too high. Mathematical economists and econometricians can do this much better and much more easily. In my view an excess of our best skills and energies are directed to creating mathematically refined models of growth. Too little addition to knowledge is coming from the more or less intricate growth models. They give us alternatives for interpreting reality that are too numerous and too wide. And there are too few acceptable criteria for a good choice to be made as between them.

On the other hand, we have the statistical pictures of economic growth, presented more or less independently of the mathematical models. Here the definitions of variables and the basic assumptions as to interdependence are more or less hidden. The statistical time series of national incomes and capital stocks invite over-bold economists and econometricians to interesting and exciting interpretations of the process of economic growth. But the gaps between the statistical results and the interpretations are wide and often impossible to bridge.

SOME ISSUES OF ECONOMIC POLICY

The modest contribution that I have to offer is partly concerned with a few elements of a growth model and partly with the interpretation of corresponding elements of statistical knowledge. My problems refer to the notion of marginal efficiency of investment, and from the point of view of the statistical picture I am seeking to interpret the notion of profitability of investment.

My researches in this field arise out of current economic policy issues in Sweden. This is nothing to be ashamed of. There is an old tradition of close connection between economic policy and economic research. Its danger is that economists are tempted to be superficial and to base realistic policy conclusions on insecure foundations.

The issues that form the background to my research are not peculiar to Sweden. There has been a great scarcity of savings during the whole of the post-war period, and we expect that we shall continue to have great difficulty in providing for the growing volume of worth-while investment. The last

[1] This article represents a shortened version of two lectures delivered by the author as Marshall Lecturer in the University of Cambridge.

five-year survey of Sweden's future economic development, presented two years ago, predicted growing pressures. On top of a rapidly rising standard of private consumption, natural in a full-employment economy with an automatic and stable process of income inflation, very important social reforms were coming: a gradual shortening of the working-week, and a radical and general improvement of the pension system. The long-term investment programme emphasised the important sectors of the economy where investment had been neglected: housing, and various kinds of public investment in hospitals, schools, roads, water and sanitation, and so on. At the same time the investigation revealed rapidly growing investment plans in the private sector of the economy, which was adapting itself to great technical changes and to expanding markets at home and abroad. The method used in carrying out the survey of future investment needs and savings was very crude and failed to consider the implied relations between capital formation, production and prices. None the less, the recommendations for a higher gross investment and savings ratio in the Swedish economy and for more " rational " methods of determining priorities for investment projects provided plenty of material for debate on economic policy.

The questions I have in mind are thus: How much does it help, from the point of view of productivity, to raise the total gross savings ratio of the economy by so and so much? Or, how much lower will the rate at which production increases be if the savings ratio is forced down? Tendencies of this kind are more or less taken for granted, but there seems to be a great deal of variation in opinion as to how powerful the effect is and how quickly it operates. The answer is naturally affected by the composition of investment. To what extent does an increasing share of long-term public investment, including housing, affect the rate of increase of production as compared with a development more favourable to investment in private industry and trade? We need to investigate the effects of allocation as between branches of industry, types of investment and firms of different kinds within a branch. Looking at the question from another angle, we arrive at the intricate problem of how and by what criteria are gross savings allocated to the various channels of investment? In Sweden, the whole problem of the functioning of the credit and capital markets in relation to the productive use of savings has been brought very much to the fore in the heated debate on pension reform. The structure of the capital market will be radically changed by the creation of the huge pension fund proposed by the Government, ultimately reaching the order of one year's national income, to compensate for the expected decline in private savings and to meet the needs for increased savings. Is this change going to be for the better or for the worse? And from whose points of view?

What can economists say about such policy issues? What criteria can we use, not to solve impossible problems but to make more fruitful a sterile debate, in which the combatants are armed only with vague principles of

sound or progressive economic policy? My intention is modest: to try to use available theory and statistics to clarify problems of this kind.

The Limitations of Simple Growth Models

I shall begin by examining some of the well-known growth models to see how far they can help us to understand such problems. I recognise that the models have not primarily been created to elucidate the particular problems I have in mind. Abstract theories of capital formation, such as Harrod, Domar, Mrs. Robinson, Kaldor, Champernowne and others have constructed, are of great importance in the development of economic thinking, not least for making precise sense or nonsense of vague statements in the discussion of economic growth. We enjoy or do not enjoy each other's models, and can give reasons for doing so. Sometimes an economist thinks that he can destroy a theory by showing that it is founded on an extremely abstract and unrealistic hypothesis that can never be tested. This is, of course, an illusion. The abstract hypothesis can be defended as being an accepted way of thought, belonging to an important line in the history of economic doctrines, and in the model these ideas are only brought out more clearly. Even if no empirical test can be applied, the features involved in the abstract hypothesis might be defended as essential to the understanding of economic growth. The omission of such features and the incorporation of other variables and relations, just possibly capable of translation into econometrics, might make the model still more unrealistic. The hypotheses underlying our use and interpretation of capital and production statistics sometimes seem to me to be more risky than the pure theories. Be that as it may, my criticism here of the models in question, with and without statistical applications, is from the limited viewpoint of how far they help us to clarify the type of economic-policy issues I have mentioned.

My scepticism about prevalent growth models refers especially to the high degree of aggregation and the concepts of capital and investment connected with this. Of course, all model builders are aware of these difficulties and perhaps fear that, if they did not simplify in one respect, they would be forced to over-simplify in another and more important respect. The prevailing modes of model building contribute little clarification to the problems of how changes in the total volume and allocation of investment may influence production, and of how to judge the functioning of the credit and capital markets with reference to marginal efficiency and profitability of investments. What, in my view, is very much needed is a sensible interpretation of capital as a factor of production and a disaggregation of the concept, or the statistics, of the volume of total capital into parts. A disaggregation is needed to clarify the problems of the *interdependence* of capital and capital investment in various sectors of the economy. We miss essential features of the process of capital formation in a developing economy *if* we aggregate too much and leave out completely the interdependence problems. Moreover,

an analysis of the interdependence problems presupposes an understanding of the profitability criteria of investment in the various sectors of the economy. No easy way out can be found by assuming some kind of profit maximisation rule. Account must be taken of administrative behaviour, of current rules of thumb, and of the permanent and characteristic imperfections of the capital market. I am going so far as to maintain that we overlook essential features of a dynamic and expanding economy if we make the assumption that a capital market is perfect, or even that it has a tendency towards perfection. To illustrate these problems, I propose to make use of some results of an interview study of profitability of investment in Swedish industry—a research project that I have been working on recently.

Let me begin with the problem of the effect of the rate of growth of capital on the rate of growth of net output. I shall use rough overall figures, assumed to be representative for the Swedish economy. If the average rate of profit on all invested reproducible capital is about 5% and if, as many investigations seem to show, the profit rate tends to be reasonably constant over time, and if there is a net rate of investment of say 10% per year, then, at a rate of growth of total net production of 2% per year, the capital accumulation should only account for one-quarter of this rise of production. 5% on the additional capital corresponds to only 0·5% of national product. An economic policy successfully promoting savings that brought up the average savings propensity so much as from 10 to 15% would with the same assumptions only augment the rate of progress by one-quarter of 1%. Very small reward indeed for this great effort!

Here I have the first and very simple answer to one of my questions: how great is the marginal importance of savings for the growth of the economy? This interpretation is obviously based on marginal productivity theory, where you can measure the importance of the factors of production from their remuneration. For the sake of simplicity I shall call this kind of interpretation the Douglas approach.

As an alternative interpretation, at the other extreme, I can take the Cassel approach giving maximum importance to capital accumulation. This is the simple exponential-growth formula used so commonly in the literature on economic growth, where the exponent is the relation between the net savings ratio and the capital coefficient. In the case mentioned above—if the marginal capital coefficient is stable at 5—the Cassel theory would imply an increase in the rate of growth of the economy from 2 to 3% per year as a consequence of the higher savings ratio (from 10 to 15%). It is clear that it is very important for economic-policy decisions whether the rate of growth of production is influenced significantly or hardly at all by a given change in the savings ratio.

More sophisticated versions of the marginal-productivity theory have often been used to give quantitative precision to the relative importance of factors supposed to determine growth of production: on the one side, input

of labour and capital; on the other side, more or less unknown factors summarised as shifts of the production function. A great number of applications of the Cobb–Douglas function have been made, and in spite of all the serious criticism directed against the method new experiments are continuously being tried. I shall mention briefly the results recently published by Aukrust for Norway and Solow for the United States, since it gives me a good starting-point in the search for alternative methods.[1]

Aukrust, using a Cobb–Douglas function, finds for the Norwegian growth of real national income 1900–56 (excluding war years) an exponent for real capital of 0·20 and for labour of 0·76 and a yearly shift of the production function of 1·8%. Applying the findings to the post-war period 1948–55, when capital accumulation in Norway indicated a Western record (5·6% per year), Aukrust finds that the capital coefficient was increasing rapidly and that the contribution of capital accumulation to the growth rate of 3·4% per year in spite of this was less than a one-third of this rate. Aukrust takes these results seriously and uses them to show that a lower rate of savings would mean comparatively little to the rate of progress. More attention should instead be directed to the factors determining the shifts in the production function, technical research, administration, investments in education.

Solow has the same problem of segregating variations in output per head due to technical change from those due to changes in availability of capital per head. The time series of private non-farm output per hour shows a doubling over the years 1909–49—in real terms. According to Solow's econometric manipulations, 80% of this growth rate is accounted for by the cumulative upward shift of the production function; the average rate of shift during the whole period was 1·5% per year, and thus close to Aukrust's results. An interesting result, according to Solow, is that the rate of shift was less than 1% on the average up to 1929 and as much as 2½% per year during the period 1930–49, when " employed " capital actually increased at a significantly slower rate than the number of hours worked. The elasticity of production with regard to capital is about 0·35 according to Solow's production function.

We all know the shaky foundations of the Cobb–Douglas technique, both statistically and as a theory of production. As was pointed out long ago, the intercorrelations of the time series in question are apt to give very unreliable results. My own very rough experiments with Swedish statistics show that the differences in the closeness of fit are often not great enough when using elasticity coefficients for capital varying between 0·20 and 0·50% for one to feel sure that the one interpretation is clearly preferable to the other. And the results as to the rate of shift in the production function would be entirely different.

[1] Odd Aukrust, "Vebst og Struktur," *Bedriftsøkonomen*, December 1957; Robert Solow, "Technical Change and the Aggregate Production Function," *Review of Economics and Statistics*, August 1957.

To this very wide range of choice as to the interpretation of accepted statistical time series must be added the unavoidable uncertainties of definition and measurement, especially of the growth of the volume of capital. The capital-volume series we have used to measure the trend of capital coefficients in Swedish industry have in my view an indeterminate range of about ±10–20% over a ten-year period; that means that we do not really know whether the volume of capital in Swedish industry has increased by 90% or 50% over the period 1946–56 (that is, more or less rapidly than the volume of production). This uncertainty is due both to lack of clear definition and to the fact that the definitions stand in an unknown relation to available statistics.

Before I examine these problems let me deal with a simpler and, in a way, clearer approach to our problem of estimating the relative importance of real capital accumulation in explaining the growth of production. This consists of measuring the rate of increase of total productivity by relating an index of the volume of production to an index of the volume of input of production factors of all kinds, including both labour and capital. The weighting becomes a delicate problem, but it is an honest index-number problem and is not concealed in a correlation procedure. It is reasonable to argue—as Professor Kendrick has done—that the relative weights of different kinds of labour- and capital-input units should be determined by their base-period prices, hoping that these approximate the marginal contributions to output during that period.[1] Such a measure should tell us how output would have developed if productive efficiency of the various inputs had been constant and the same as in the base period. And the relation of the two index series of actual and hypothetical output gives us the rise in productivity that has to be explained by other factors than changes in the volumes of labour and capital input.

I regard this method as preferable from several points of view. Discussion depends too much on the very crude and much misused concept of " productivity of labour," in which the output index is related to only one of the factors of production. One can argue that as a measure of productivity an index also taking into account the inputs of capital must be " better " in some way than the usual index neglecting the process of *substitution* of capital for labour. The results are, moreover, of interest in a discussion of economic policy; that has been my experience when it could be shown that the " true " rate of increase of productivity in Swedish industry during the post-war period has not been 3·5–4% as commonly maintained but only about 1% per year. It is especially interesting when comparing rates of productivity increase in agriculture and industry, as well as in different branches of industry. The results of such comparisons between branches seem more relevant when some account—however approximate—is taken

[1] John W. Kendrick, " Productivity Trends: Capital and Labour," *Review of Economics and Statistics*, August 1956.

of the very wide differences in the rates of capital accumulation. The Kendrick approach, although in a way less ambitious than the Douglas approach, does not, of course, eliminate the fundamental questions of uncertainty as to the interpretation of the results. The questions are how shall we interpret the changes in volume of capital input; what sense there is in the choice of the relative weights for capital and labour inputs: the necessity or advantage of assuming a given production function that is implied equally in the Kendrick approach. These questions are obviously closely interrelated.

Volume of Capital Concepts

Let me start with the statistical problem of measuring the development of the volume of capital for a branch of industry or for the economy as a whole. Goldsmith and other specialists in this field, who produce measurements over long stretches of time, apparently care little about the arbitrariness of the results and the uncertainties of their interpretation. Take the problem of the deflators—hardly even mentioned as a difficult problem in the account of the methods. When using an aggregate of machinery price-index numbers for deflating fire-insurance values of capital or accumulated values of machinery investment in Swedish industry I had a very uneasy feeling of the uncertain nature of the underlying machinery price statistics. Do the prices of the machines included in the index take account of changes in the efficiencies of new machines—and if so to what extent? My belief, based on some acquaintance with our machinery price-index numbers in Sweden, is that the collectors of the price material try to include only standard machines with constant efficiency in some sense, using chain links to make adjustment for necessary changes of types. But I am sure that they do not succeed very well in this endeavour. The index therefore gives not the price of an unchanging " volume " of machinery services, but rather prices of machines with a rising trend of efficiency. If the efficiency of the machines is rising on average by, say, 1 or 2% a year, the use of this type of deflator gives a lower rate of increase of the machinery capital interpreted in productive capacity terms than a " correct " index would give. I dare not raise the question of how effective the machinery price-formation process is from the point of view of difference in quality and efficiency. Generally there is a tendency for an increase in efficiency of a new model that is greater than the rise in price. There is the same type of bias in the measurement of the volume of construction. A construction-cost index with relatively fixed weights tends to over-estimate a rise in building costs, and a rising efficiency of the use of factory space is, of course, not accounted for. The sad thing is that the statisticians do not really know what they are doing when they deflate the capital-value figures, and the economists do not know which capital concept the volume estimates most closely approximate.

This brings me to the difficult problem of the relation between the interpretation of statistical time series and model builders' conceptions of

essential features of economic reality. In the production function approach, the theorist usually likes to consider capital as input of some kind of original factor of production, measuring input as costs of labour and materials embodied in the machines or buildings in question. An assumed production function gives precision to the notion of how much an increased volume of capital should add to production under given technical and other conditions. The rate of increase of " total " productivity—the shifts in the production function—represents our *ignorance* of the causes of economic growth.

It is, however, possible—as Kuznets has maintained—to define the volume of capital with regard to the capacity to produce embodied in the machines and buildings.[1] Difficulties as to time dimension and degree of utilisation are the same in both cases and can be assumed away. This means that if a machine costs as much as in a base period, in terms of input of labour and other real costs, but at a given rate of utilisation renders 50% more in the form of flow of services (for example, in horse-power), then there is a corresponding increase in the volume of capital. From the point of view of the reproduction cost of an existing capital structure, old capital goods should then be imputed values with regard to the cost of replacing them with modern machines built with improved techniques but giving a constant volume of services. Capital consumption may according to this approach be defined as the scrapping of old equipment with no deduction for obsolescence as long as capital instruments are repaired and maintained to keep constant capacity. This means that the depreciation charges of old capital will be probably lower than according to the input-cost approach (for example, the perpetual inventory method) and also that the rate of growth of the volume of capital will be more rapid. It may be significant that Kuznets' index figure for the growth of reproducible wealth in the United States over the period 1900–50 lies 30% above Goldsmith's measure.

In pure models the one approach can logically be made as precise, attractive or persuasive as the other. And there are no *a priori* criteria as to which model is less realistic or more difficult to apply. What I am interested in is the shift in interpretation of the rôle of capital formation implied. In the first approach our attention is directed towards the substitution possibilities between labour and capital under given static conditions—and, as the statistical applications seem to show, a relatively small part of growth is explained by this method. In the second approach investments are somehow supposed to materialise in capital goods of rising technical efficiency and productive capacity, with the result that a Kendrick method of analysis would result in a lower rate of unexplained rise of productivity. There would be correspondingly fewer " unexplained " shifts in a production function. It is evident, however, that the difference between the two types of approach means a transfer of ignorance from the unexplained shifts of the production function to the hypothetical efficiency effects of investments in

[1] See the discussions in *Problems of Capital Formation*, Studies in Income and Wealth, Vol. 19, 1957.

new capital goods which are transposed to a capital-volume index. One important consequence of this latter approach could be that capital accumulation would be regarded as a more important factor of growth. This is on the condition that when the rising efficiency of capital goods is incorporated in the definition of capital volume, the weight of capital can be assumed to be the same, independent of the definition and measurement of capital growth.

Some More Sophisticated Theories

At this point I come very close to profound problems intensively discussed at a high theoretical level here in England. I am especially thinking of the theories and definitions developed by Mrs. Robinson, Mr. Kaldor and Mr. Champernowne.[1] I am not competent to judge the intricate details of their models—and to what extent the results may clarify my problems. As far as I have understood the issues as to capital formation involved in the discussion, a fundamental dilemma resulting from the application of classical tools of analysis is clearly unveiled. The production—or for that matter the productivity—function is a useful instrument for showing a substitution process of capital for labour under static conditions—where technical knowledge is given. The concepts of capital volume and marginal efficiency or productivity of capital can then be given a precise meaning. When the object of analysis is widened to include discontinuous or continuous changes in applied technical knowledge, the assumed or given shifts in the production function limit the explanatory value of the theory very much. Widening the system to include these dynamic changes will, on the other hand, upset the system of explanation a great deal and spoil the simple definitions of capital and marginal productivity. The growth of capital volume can strictly be regarded as an accumulation of net investments only during the substitution process when real wages are assumed to be constant. The introduction of important technical innovations upsets the equilibrium, causes serious changes in income distribution and profit expectations and makes revaluations of existing capital equipment necessary. But the rules of valuation must be rather arbitrary, if the disturbance means an upsetting of the equilibrium conditions. In any case this type of approach implies changes in capital values that are not regarded as consequences of net investments. The marginal efficiency of investments defined as the profitability of substituting capital for labour assuming given technical knowledge becomes something very different from the marginal productivity of capital. This concept should then refer to more or less arbitrarily determined changes under dynamic conditions in the values of capital and net output, both in real terms. Of course, energetic model builders overcome a dilemma of

[1] Joan Robinson, "The Production Function and the Theory of Capital," *Review of Economic Studies*, Vol. XXI (2), 1953–54; N. Kaldor, "A Model of Economic Growth," Economic Journal, December 1957; D. Champernowne, "Capital Accumulation and the Maintenance of Full Employment," Economic Journal, June 1958.

this kind by introducing new sets of assumptions and by finding consolation in points of dynamic equilibrium.

My objection to the type of model Mr. Champernowne has built with great skill refers to the unnecessary extra complications that arise because of the artificial position of the static production function in a system explaining dynamic economic change in the economy as a whole. The term "artificial" is used as a subjective-value judgment. You can defend the production function as an important tradition in economic thought; making use of it is thus good for pedagogical reasons. A model is apt to be accepted if it contains traditional relations such as consumption and production functions, and part of our profession consists of training our skill in handling these tools for nearly all purposes. It may be argued that the production function is not more unrealistic than the Keynesian consumption function for model-building purposes. My contention is merely that the way of posing the problems that is implied in the production function does not clarify the issues I wish to have clarified.

My simple premise is that the investment process can be understood only when the reference basis is an economy perpetually out of static equilibrium. There is a more or less continuous flow of disturbances brought about by technical change as well as by other changes in demand and supply conditions. The resulting disequilibrium positions create investment opportunities, and then the investment process is continually creating new disequilibria. Only a minor part of the investment problem can *a priori* be regarded as a static substitution problem. If this view is accepted as plausible, then an effort to use the production function as an instrument to explain long-term growth must be considered as very inadequate. Solutions along these lines will from this point of view seem artificial. But of course I cannot prove this.

In Mr. Kaldor's model of economic growth [1] the distinction between movements on a production function based on a given technical knowledge and shifts of such a function is abandoned. Instead, he introduces a postulate of a given relationship between capital accumulation and growth of productivity. Instead of the traditional assumption of the production function he makes the heroic assumption of a given curve—the technical progress function—presuming a constant flow in the rate of new technical ideas and possibilities—and lets the rate of capital accumulation determine to what extent the technical opportunities can be used. In this way investment is completely and inseparably combined with the dynamism of technical change. A mutual interdependence is pictured: the speed with which an economy can absorb capital growth depends on its technical dynamism at the same time as the speed at which technical innovations can be applied depends on the supply of savings.

[1] See reference above. I am sure that Mr. Kaldor has built many new growth models since 1957.

In spite of the high level of abstraction, I consider this approach based on the technical progress function as being more promising for an understanding of the rôle of capital formation than models built on the static production function. In this discussion I am not interested in Mr. Kaldor's ambition to find a solution to his system of long-run equilibrium growth. I am only a little sorry that it compels him to introduce a number of simplifying assumptions that make his model impossibly remote from the level at which I need help to understand reality.

What explanatory value can we find in the highly abstract technical progress function and Mr. Kaldor's method of relating it to capital accumulation? It shows that productivity would grow even at zero rate of capital accumulation. This constant in the technical-progress function corresponds somehow to the shifting of the static production function; productivity will rise for a number of reasons independently of capital accumulation. What is left unexplained should, however, be less than in the Douglas or Kendrick approaches. Again the position of the curve will depend on the definition and measurement of capital and its rate of growth. The more " gross " we define investment in order to include efficiency gains when replacing old capital goods, the smaller will be the increase of productivity explained by factors other than capital accumulation. But something should always be left to be accounted for by a multitude of factors, such as improved skill and organisation, cost reductions of various kinds, enlargement of markets; all these are factors contributing to more efficient methods of production without there being any need for capital investments of the type here discussed. The continuous process of growth of technical knowledge, shifts in demand functions, price and cost changes, must mean that firms are constantly out of equilibrium, that is, with a below-optimum allocation of resources. Lagging adjustments of production and sales policies—the lags being partly determined by costs of administration, lack of information, or absence of incentives—will mean a stream of efficiency gains at the various points in the economy, which may be assumed to sum up to a yearly increase in productivity for the economy as a whole.

Interview studies in Swedish industry give me a strong impression of this factor; managers and engineers know that all the time there are a great number of suboptimal disequilibrium positions within a firm. At an interview the management of a large engineering firm maintained that at nearly any stage in production costs could be reduced by about 10%, without there having to be any investment, after a close scrutinisation of the methods of production; but there was not sufficient time and administrative staff available to catch up with development. I have obtained some sort of empirical evidence of these possibilities from a steel-producing concern. In one of its plants no fixed investment at all was made during a ten-year period; there were only expenditures for repair and maintenance, while in the other plants of the concern new investment for expansion and modernisa-

tion was considerable. The rate of increase of production per man-hour for the concern as a whole varied between 4 and 5% per year, and was about 2% in the steel plant without investment. I have several similar instances of this phenomenon, but certainly not enough evidence to generalise.

The most interesting property of Mr. Kaldor's model of growth, from the point of view of my problems, is that the marginal social efficiency of investment, as it perhaps could be derived from the technical-progress function, has very little to do with the average level of the rate of profit. The effect on production of a change in investment depends upon the slope of this function at the point in question, and this slope is determined by the investment opportunities created and by the flow of technical innovations. The sad part of my interest in Mr. Kaldor's model is that this slope, which might give me a beginning to an answer to my question regarding the effect of an increase in the investment on the rate of growth, is not revealed in any outward manifestations of the working of the model: not in the rate of profits, nor in the rate of progress, nor in the inverted capital coefficient. Changes in profits act only as short-term regulators of savings—and even in the long-run the dynamic equilibrium rate of profits on capital has no clear singular relation to marginal efficiency of investments. Is this a peculiarity of Mr. Kaldor's model or something essential to be found in economic reality?

My contention is that this lack of relation between actual *ex-post* profitability of investments and social as well as private marginal efficiency of investments is an important feature of reality and should be demonstrated in a good model. You cannot regard actual profits on capital as even approximately measuring the effect on production of changes in capital volume as the applications of marginal-productivity theories try to do. It is always dangerous to pick out and isolate one relation within a system of interdependent variables and draw conclusions of causal significance. To the extent that profits are supposed to be determined in a set of equations, then the *ex-post* profits registered may have very little to do directly with a change in the volume of capital in a production function. The same holds, of course, with regard to changes in the degree of utilisation of the capital stock.

On the other hand, I feel very critical as to the *way* in which Mr. Kaldor deals with the interdependence of capital formation, profits and changes in the marginal efficiency of investments. Very many simplifying assumptions have, of course, to be made in order to make the model manageable. The entrepreneurs seem to be very short-sighted in his model, as they are not aware of the profitabilities of investments that the technical-progress function implies. Investments are determined mainly according to the acceleration principle—with some regard being paid to *ex-post* profitability of capital —and the entrepreneurs are continually being surprised by greater increases of productivity than they had expected (on the condition that they are to the left of the long-term equilibrium point, otherwise the surprises will be in

the opposite direction). In a way, it is strange that when the flow of investment opportunities is given as being very much dependent on the growth of technical knowledge—and this is a realistic feature of the model—the entrepreneur should be more or less blind to these opportunities and only react to the expansion of the market that has just occurred. In fact, the entrepreneurs and their staffs of technicians and engineers will really be more or less busy creating small pieces of the technical progress curve and they must therefore be aware—although, of course, in varying degrees—of the opportunities for investment arising from technical innovations. This is self-evident, or interesting, or startling, depending on the extent to which we have taken a model seriously and been prisoners in its cage, or have fallen in love with it.

Some Empirical Evidence on Capital Growth

My proposition, then, is to consider entrepreneurs as experts on the technical-progress function on the micro level for the individual firm. Many of them know a lot about it, although in varying degree in different firms. Imperfections and uncertainty in knowledge have to be examined. In a model we have to make precise assumptions about these things. My contention is that by studying the profitability calculations and expectations of returns from different kinds of investments in the business firms we might find out something about the slope of Mr. Kaldor's technical-progress function. We cannot avoid meeting on this roundabout route the insoluble problems of how to aggregate the micro relations of private marginal efficiency of investments to macro concepts of social marginal efficiency relation for the economy as a whole. My point is, however, that a break-up of the aggregates is needed in order to see what problems we are really assuming away.

In the study mentioned above of profitability of investments in the Swedish economy, I find it fruitful to pursue the analysis on the macro and micro level simultaneously. The aggregated statistics of capital, investments, profits and production in total industry and in the various branches of industry can be much better understood and interpreted if intensive studies of how corresponding things happen and how they are expressed in statistics are made on the level of the individual firm. In the aggregates important features of the story may be eliminated. Misinterpretation of the statistics may be avoided if we know how things occur on the firm level and not least how the statistics are reported and created. Of course, there are great risks and much waste of time with interview studies. The interviewing of something like 100 industrial firms is apt to give a bias. The selection of firms has in my case nothing to do with a well-structured sample. It is instead heavily influenced by such irrelevant considerations as that the firm is relatively big and the managers are open and interested in the problems in question. The interviews have been partly of a more extensive kind in order, for example, to get a general impression of the extent to which profitability

calculations have been made, and if so by what methods. More intensive personal interviews have been made of about twenty companies. On some occasions it has been possible for me to take part in the profitability discussions of the management of the firm when investment decisions were being made.

What can be the outcome of such small-scale interview research? I am not in a position to apply significance tests to the results. My intention is only to survey the types of problems that an economist meets when he tries to understand problems of marginal efficiency of investments from an *ex ante* point of view. For my own part I have been learning a great deal about criteria for investment decisions—and that is certainly an important element in this kind of direct observation. One clear outcome is that my propensity to generalise from official statistics and oversimplified macro-models is much diminished.

Let me begin with some comments on the measurement of the growth of capital volume and of capital coefficients in Swedish manufacturing industries. The official statistics of fire-insurance values, deflated by the special machinery price and construction-cost indices used by the insurance companies, give as a result capital coefficients varying from 1–2 up to 6 for the various branches of industry. Looking at individual firms inside the branches we also find great dispersion. If agriculture, power production and housing are included the dispersion is, of course, still wider—up to 15. All averages for the economy as a whole must therefore be closely watched with regard to changes in the composition of the national product. For manufacturing industry as a whole, as well as for the various branches, we generally find higher marginal than average capital–output relations.

Capital volume, according to our measurements, has tended to increase more rapidly than production volume during the post-war period, and indeed from the end of the 1930s if one may trust very rough estimates. Barna seems to have reached similar results for England.[1] How significant are such results? The problem is important from the point of view of estimating future need of savings for economic growth.

I have already mentioned the difficulties of price indices and definitions of capital volume. I shall only add that alternative measures of capital volume, namely the index of horse-power installed and the perpetual-inventory method, gave so much lower growth rates that the conclusion about rising capital coefficients is in any case rather doubtful. Regarded as an efficiency or capacity measure of capital growth, fire-insurance value may be more significant than as a measure of capital input. In our interview studies we tried to check the significance of the fire-insurance statistics. We drew graphs showing development of fire-insurance values, cumulative figures on gross investments in real terms, production, employment for firms

[1] T. Barna, "The Replacement Cost of Fixed Assets in British Manufacturing Industry in 1955," *Journal of the Royal Statistical Society*, Series A, Vol. 120, Part 1, 1957.

interviewed and inquired about strange deviations in the curves. It was a fruitful way of finding out sources of errors. The general impression from these discussions was that fire-insurance values tended to overstate reproduction costs of reproducible capital (after allowance for depreciation) but not by very much, but that the trends of growth could be treated as significant for capacity development. It was, however, disturbing that at intervals of five to ten years there was usually a complete revaluation of the fire-insurance values—and it cannot be judged to what extent these revaluations tend to average out in the aggregates.

One possible method of answering the question of whether capital coefficients tend to be rising in Swedish industry would be to try to compare capital–output relations derived from fire-insurance statistics with corresponding relations derived from expansion programmes planned or in process of realisation. But here we meet many interesting difficulties. Obviously one must not compare planned investment expenditures with expected value added; to make the figure comparable with *ex-post* relations based on fire-insurance and production statistics, the investment figure must be reduced to some kind of average value of real capital over the life of the investment. The arbitrariness of any such deduction leaves little confidence in a comparison with *ex-post* average and marginal relations based on fire-insurance values. The few comparisons we have been able to make seem to give low capital–output ratios for new expansion programmes as compared with existing structures.

One must be very careful in drawing conclusions, however. An expansion is usually based to a varying but sometimes quite important extent on the use of already existing facilities of the firm—power supply, transportation equipment, warehouses—where there is unused capacity. This is particularly the case when old buildings can be used and the expansion refers only to machinery and equipment. The need for new factory building may come later when higher profits at the expanded level of production make financing easier. To this factor is due a not uncommon tendency to underestimate *ex ante* the total cost of investment, not least because of an unexpected rise in the need for circulating capital.

The capital needs of an investment project are only one part, and certainly the easiest part, of a profitability estimate. And it is on this point, as I have said, that I like to use the entrepreneurial expert knowledge to obtain some basis on which to judge the slope of a Kaldorian technical progress curve, or of Keynes' marginal efficiency of capital. Let me start again with the *ex-post* values. On average for the past five years I find a level of gross return on capital of 8–10% before taxation in Swedish manufacturing industry, corresponding to perhaps 5–7% net return. This level of *ex-post* profitability in Swedish manufacturing industries is much lower than Barna's very similar estimate of about 15% gross profitability for British manufacturing industries—as presented in his address to the Royal Statis-

tical Society in 1957. But I think Barna tends to overestimate gross profits considerably through his method of making subtractions from value-added figures. Using Barna's method, I also get 15% gross profits in relation to total assets. The reason is that all items of cost are not accounted for in the production statistics. My results are based on official statistics of profits that can be made approximately comparable with the statistics of fire-insurance values, and I think this method is a little more reliable. A low *ex-post* profitability on invested capital is also a phenomenon I very often find in the interview studies of the individual firms. I have quite a few examples of rates of net return approximating 5% (before taxation) on reproduction values of existing capital, and this was during the very good years 1955–57, and refers to corporations which were probably among the more efficient in the respective branches.

The best way of valuing capital will always remain a puzzle. The book values of the companies' own capital (share capital and funds) were very much lower and yielded much higher returns. To the extent that the reproduction cost valuation measures costs of re-erecting the capital structure of the firm with to-day's costs of reproducing old capital goods, or substituting with new capital having the same efficiency as the old ones, it is quite natural that net *ex-post* profitability shall be relatively low. To what extent fire-insurance values or expert evaluation approach such an indeterminable principle of valuation can never be very well ascertained because of the lack of clear criteria. The unsatisfactory feature of this kind of measure of profitability is that we are again trying to introduce a pseudo-static reference scale in a world of dynamic change, in which essential characteristics of the conditions of capital formation cannot be properly understood if static notions are preserved.

Profitability Estimates

I have dwelt on this notion of *ex-post* profitability of capital as it constantly arises in the discussion of economic policy. Representatives of private industry come back to calculations of this kind to show that Swedish industry cannot, after paying very high corporation taxes and dividends, even finance reinvestment of old capital. It is maintained that the very sound consolidation that is characteristic of the greater part of Swedish industry is a result of a long sequence of exceptionally good years of inflation. At prevailing profit margins and with the prospects of less inflation and more competition, determined among other things by the European free market, the outlook for profits is much less favourable. My own view is that industrialists in the political arena always tend to be pessimistic. As investment experts, planning for the future, they are apt to be more optimistic. I think that the presentation of some results of my interview studies will clarify the significance of such doubletalk.

I can summarise my impressions from the interview study under three

headings: first, the relatively low frequency of more exact profitability calculations for new investments; secondly, the wide differences in definitions, methods and basic assumptions in the calculations and the difficulties of translating them into some common " language " or norm of evaluation; thirdly, the high dispersion of yield expectations from the various kinds of investment within a firm, as between firms in the same branch of activity and as between different sectors of the economy.

Observations under these headings are, of course, closely related. Behind all the difficulties of comparison lies the subjective nature of every estimate of uncertain future costs and revenues. How and to what extent consciousness of the uncertainties of the future is included in the profitability estimates must be a very subjective judgment. Investment decisions have in any case to be made on uncertain and insufficient premises. Whether a more exact profit estimate is formulated or not may depend on such circumstances as the education and training of the entrepreneur and his men, the traditions in the firm, the effectiveness of an exactly formulated profitability calculation as an instrument of persuasion among the members of a board of directors and so on. The profitability estimate may in many cases have very little to do with the investment decision. Some kind of loose estimate and weighing up of possibilities is always done, although a quantification of the hunches may be regarded as unnecessary for decision making. In the interview studies I have tried to find out something about the frequency of profitability calculations with regard to various types of investment and branches of activity. An account of the results would, however, lead me too far from my main subject.

My own bias in favour of the view that more exactly formulated profitability calculations can be useful in a study of this kind should not be forgotten. My doubts and reservations will soon come. Let me first present some results. As I have said, the most surprising discovery to me—at least in the first stages—was the great dispersion in the expected internal rate of return (before tax) of planned investment projects. During the period covered, the good years of 1956–57, investment decisions were made on the basis of internal yields as low as 4–5% and as high as 70%. Yield expectations around 20–30% were quite common in manufacturing industry; but much higher and much lower returns were expected quite frequently. Investments in electric-power plants were generally expected to yield between 6 and 8%, while the figure for house-construction was around 6–7%.

There is not only a wide dispersion between branches of activity and between firms but also within a firm. In a big pulp- and paper-producing corporation the internal yield expected on investments actually being carried out varied from 3–4% up to 30–40%. Investments in forest plantation were being made on a very low profitability basis, while machinery purchases for modernisation and automation purposes showed very high expected yields. This observation of great variability in returns on actual investments within

one and the same firm was quite general. In a firm there always seem to exist many " golden " investment opportunities in the form of new labour-saving machines to replace old ones, of methods of saving fuel, investments to expand bottlenecks and the like, promising very high and quick returns. The responsible engineers have to be restrained by rules preventing them from buying machines that have a longer " pay-back-period " than one, two or up to five years—depending upon the firm's financial and administrative capacity. The yields will, of course, depend upon the expectations of the length of economic life of the machines in question, but mostly the returns will seem very high in comparison with the expected returns on investment in long-term expansion programmes. And these expansion programmes are given priority although they are only expected to yield a 10 or 20% rate of return, compared with a stock of " golden " investment opportunities yielding perhaps 30–50% on a comparable basis of estimate. These highly remunerative investments are squeezed out in order to make room for the financing of the expansion programme. Over a ten-year period companies have to make quite substantial investments in welfare or community projects of various kinds with no expected direct yield at all, but with opportunity costs corresponding to the postponed or neglected golden investment opportunities.

On this level of direct observation we obviously get a very chaotic picture of economic reality. This is nothing new; economists working on investment problems have always known that there were wide ranges of yield expectations. To present figures of doubtful significance may be considered irrelevant. But it is one thing that economists should know it. It is another that they should assume it away or forget it when they build their models. We may admire the courage of the economist in making order out of chaos by assuming a natural rate of interest or a schedule of marginal efficiency of investment, or a perfect capital market. I suggest that we should, to a greater degree than we usually do, accept this chaos on the capital market and, instead of abstracting from it, analyse the causes and effects of very high dispersion of expected investment returns. It is from this angle that we should try to understand the functioning of the capital market and the relatively low *ex-post* returns on capital.

The Interdependence Problem

There are many difficult problems connected with the interpretation of business-men's expectations of net yields of investment. I leave aside the niceties of financial mathematics as secondary. I would only remind you that to summarise the economic prospects of an investment by giving only the internal rate of return neglects a number of considerations. I also leave aside the question of the compatibility of subjective biases due to the temperament of the estimators: some estimate hopefully, others conservatively; we have here all the various methods of transforming doubt into certainty, subjective ways of including contingency allowances in price and cost fore-

casts, in estimates of average degrees of utilisation of capital, in estimates of time-lags between investment costs paid out and the full operation of the plant and the length of its economic life.

Allow me in spite of all this to assume the business-men, making profitability estimates, to be experts on the real rate of interest and the marginal efficiency of investment. Some correction for apparent differences in methods and assumptions can be made, but of course never on a perfectly objective basis. The results will, I am sure, still show a picture of very wide dispersion in expected net returns on investments. My intention when leaving out these, I admit, somewhat disturbing questions is to concentrate attention on three issues of great importance in the interpretation of profitability statistics: the interdependence of investments in various fields of activity and therefore of returns on investment, the dynamic effects of investment on income distribution, and the influence of costs of capital looked upon from the point of view of the capital market.

The question of interdependence explicity or implicitly enters into every investment problem. In the simplest form the bank meets it when, for instance, scrutinising the credit-worthiness of a firm's machine-investment. It often happens that a very fine profitability estimate is presented, but that the bank manager has to point out that a considerable amount of supplementary investment is needed to finance increased circulating capital. It is not uncommon even in big corporations that the investment cost of an expansion programme is underestimated considerably because of an underestimation of the needs of increased circulating capital as well as of more of additional credits to customers and greater needs of liquidity. In the case of an iron and steel plant, to mention only one example, an expansion programme extending production capacity by about 50% implied needs of increased circulating capital corresponding to about 60% of planned expenditure for fixed investments in factory buildings, plant and machinery.

But the interdependence problem goes deeper than the mere accuracy of estimates. High profitability of particular machinery investments in a firm is often dependent on some surplus capacity of plant, buildings or transport equipment; this is, of course, most clearly the case when a bottleneck is eased. When ranking investment projects from a short-run point of view this is right. But from the point of view of the balanced growth of the firm in the longer-run this kind of direct profitability estimate is less valid. The golden opportunities of bottleneck investments are dependent on the general development of the firm and the profitability prospects of the firm's total capital. It may, therefore, be more profitable for the firm in a given situation to postpone investments which seem much more profitable than the long-term investment programme. In this case it can be said that the profitability estimates are not accurate guides, either for the investment policy of the firm or for the economist trying to understand something about the marginal efficiency of investment. The very low return on investment

in forest-growing in the pulp and paper companies mentioned cannot be analysed as an isolated factor. Current high expected returns on machinery investments are to a larger extent determined by long-term investments in the forests made thirty years ago.

The nexus of difficulties in estimating net returns that I am here approaching is evidently closely related to the classical problems of internal and external economies. When a big aluminium company was interviewed about expected returns on a planned expansion programme they appeared promising, giving something like 20% net internal yield. But this profitability estimate was based on unchanged costs of electric power. After revision of the estimate to take account of investment costs for the additional requirement of power, profitability turned out so much lower that, according to the manager of the company, the expansion programme would not have been considered on the basis of this latter estimate.

We meet here the strategic interdependence of public and private investments, which is especially important in Swedish economic policy discussions. There are many complaints from private business about the rapidly rising trend of public investment—including government-regulated house construction. The public sector, in a wide sense and including house building, now takes about 60% of total gross investment of the Swedish economy, squeezing private investment in industry, agriculture and trade to about 40% of the total. It is pointed out that, according to the national accounts, the private sector—taking 40% of total gross investment—makes a contribution in the form of value added corresponding to 80% of national income.

I need not point out the sources of errors and dangers in such a comparison. What I would like to stress in this connection is the dependence of the relatively high net returns (before tax) of private investment on the large public investment expenditures that generally yield low or no returns. I find it a little difficult sometimes, however, to persuade members of Chambers of Commerce and Bankers' Associations that they should be grateful to the Government and the municipalities for their generosity in taking over so much of the economy's fixed capital investments, having very high capital coefficients and low or no returns—thereby making private investments more profitable than they otherwise would be.

An interesting feature of this problem of interdependence of public and private investments is that this interdependence operates differently in various sectors of the economy. Quite a large part of Swedish industry is located in the country-side, in small towns, where often one or two industrial companies (iron and steel works, wood industries or engineering companies) completely dominate the community. The investment costs in an expansion programme of such a company must often include quite a considerable amount of community and social costs, which in a large city would form part of public investment. Our taxation system does nothing to correct any bias in profitability estimates as between companies situated in small towns and big cities:

The main point, however, is the varying difference between the estimated profitability of private investments taken as an average for branches of activity and the calculated social marginal return on the corresponding investments when account is taken of the need for complementary public investments. And exactly the same consideration comes in when studying the interdependence of investments within a firm and between branches of activity. One way out of these complications, which is really a retreat from the difficulties raised, is to aggregate investment and returns for the whole economy and look at the results *ex-post*. We are then back at the aggregate capital theories that I discussed earlier.

Disequilibrium Positions and Profitability

My theme is, in fact, that if we wish to understand the dynamic processes resulting in the aggregates we have to pay attention to the wide dispersion in the expected profitability of investments at the various points and sectors of the economy. From this point of view, investment in capital goods normally occurs in partial disequilibrium situations, characterised as promising investment opportunities. The essential characteristics of these positions are determined by the possibilities of introducing new methods of production, new types of goods, the creation of new markets, replacing obsolescent machines by new and more efficient ones and so on. The relatively high profitability is to be explained as a result of partial disequilibrium. It may be assumed that equilibrium positions are in some way approached at the various points in the economy as a consequence of the increased production and productivity following the investments, but new disequilibrium positions are continually created, partly as a consequence of this very flow of investments. New demands arise in lagging private and public sectors of the economy as well as within the individual firms, and obsolescence of old capital is created. The flow of new technical knowledge being made applicable for use in production is more or less continually creating new disequilibrium positions and investment opportunities. This Schumpeterian way of looking at the process of economic development gives, of course, only a limited aspect of the conditions of capital formation—certainly very far from a complete model. There are a number of other conditions, relating, for example, to total demand and employment that have to be considered in a complete system of explanation. But for the moment I am concerned only with one element in the model.

I come now to the income-redistribution complication of capital formation that is in fact related to the interdependence problem. I have constantly met this problem in interviews on the micro level. The question arises about the price and cost estimates and the assumptions about future development that are built into or implied in the profitability estimates. Should the estimate be based on the price and wage cost relations ruling at the time of the investment decision, or should the estimated return include a

forecast of the probable changes during the economic life of the investment in question? I have come across very different attitudes to this question, partly depending on the purpose of the profitability estimate. If the purpose is one of ranking potential investment projects of similar type as to profitability at a given time, then it does not matter so much if to-day's or some future price—and wage—relations are used. The relative positions of the projects will usually not be much affected. When, however, the purpose is also to use the estimate of the returns on investment for forecasting the future financial positions of the firm, attention has, of course, to be paid to the probable trends of wages and prices.

I can again take as an illustration the profitability estimates of the investment programme of the iron and steel plants mentioned earlier. On the basis of wage costs and prices ruling in 1955, the gross profits before tax would in 1963, when the new capacity is expected to be in full production, have increased by about 40 million Swedish crowns. When, however, account had been taken of an expected rise in wages per hour, in relation to steel prices, of, on the average, 3% per year—this being the trend observed for the past years—the increase in gross profits would be reduced to 15 million crowns. This is 60% less than the hypothetical amount at given price and cost relations. Under reasonable assumptions as to the length of the investment period and the economic life of the new plant the internal rate of net return would be nearly 30% in the first case and only about 10% in the second case, when account is taken of the changed wage–price relation. If wages continue to rise in the same way during the whole expected lifetime of the investments the internal yield will ultimately be reduced to 1–2%. This is, of course, a very unrealistic example, being an *ex-ante* calculation of the *ex-post* profitability results under given productivity development. Unexpected golden investment opportunities may improve the profitability results and make the second alternative the more realistic. But you will notice the unavoidable arbitrariness in the methods of forecasting total results, when account has to be taken of the interdependence of yields of the firm's investments over time as well as of the impact on costs and revenues of general economic development.

These are problems which are discussed on a very high level of abstraction in Mrs. Robinson's and Mr. Champernowne's theories. The rise of wage costs in relation to capital costs during the course of development and the corresponding redistribution of incomes and the process of revaluation of capital give rise to serious and difficult problems in capital theory, especially when they are handled with the old tools of analysis. In terms of the division of labour that I have suggested, my limited task is to enlarge the inventory of different aspects of profitability in the light of my contacts with reality.

There are clearly other types of changes of income distribution than those that I have just mentioned which can be foreseen and built into a profitability estimate. Instead of wage earners directly absorbing the

benefits of the productivity increases consequent on investment, consumers in general may receive the benefits by way of price competition. Investments in new technical processes or in the production of new commodities create quasi-monopoly positions, promising high returns in the short-run before other producers enter the field. I only need to remind you here of how intimately profitability estimates of investment, both *ex-ante* and *ex-post*, are related to market structure, price policy and wage formation. In this connection, I have to ask how anticipations of market reactions enter into profitability estimates and how they should enter if they are to throw light on the marginal efficiency of investment.

I quite often find that these problems are in some degree taken into account in the sense that firms anticipate about the same gross profit margins on sales as the average that has been ruling during past years. This implies that enterpreneurs expect that exceptional investment opportunities, promising high profitability, will not usually last very long. Increased wages, lower prices and other forms of competition will sooner or later eliminate exceptional gains. And for the longer run there are also expectations that, with costs of capital and labour about the same for their competitors, lasting deviations from the anticipated relations will be impossible. From another point of view, this prospect of future changes in price–wage relations will be interpreted to imply bankruptcy in the longer run for a firm that fails to make investments to secure higher productivity. Some kind of alternative calculation showing the difference in net returns between making investments to secure higher productivity and failing to make such investments will again result in the same kind of high profitability that emerged under the assumption of given price–wage relations.

What I am trying to say here is closely related to the problem of *ex-post* profitability that I discussed earlier from an aggregate point of view. The iron and steel plant to which I have been referring as a micro model made a calculation of the costs or reproducing the whole plant at the 1957 price and cost level. The result was a capital coefficient of 5·5, in fact very close to *ex-post* estimates for the iron and steel industry as a whole, built on fire-insurance values. In the calculation the higher efficiency of a newly built capital structure was eliminated by assuming such a rise in wage costs that gross profits remained at a conservative level of 15% of the value of production (it is only a little more than 10% to-day). The result was a rate of yield on capital of only 3%. With a reasonable capital structure, a long-term interest rate of 5% and some normal consolidation after taxes at 57% on net profits, the corporation would be able to pay 1·3% yield on its share capital. I have examples of similar tendencies for other corporations interviewed.

Summary

Let me now summarise the lines of thought that I have been illustrating. There is a series of alternate investment profitability concepts of varying

significance: first, profitability estimates of an investment in an expansion programme at given prices and wages; secondly, the same after correction for an autonomous rise in productivity, supposed to be possible without investment; thirdly, profitability of an investment with due regard paid to an expected rise in wages in relation to prices; fourthly, some kind of measure of *ex-post* profitability of the total reproduction cost of capital that is invested in a firm.

The fourth definition, usually giving a very low yield, is sometimes used in economic policy discussions to show that Swedish industry, at prevailing tax rates, labour costs and the rest, is doomed to ruin. After paying taxes and dividends, industry cannot afford even to replace worn-out capital; this is the tragic conclusion of this type of argument. And yet these entrepreneurs sitting at their desks are quite different people from what they are when they are putting their case in the political debate. And quite rightly so. The *ex-ante* expectations of profitability are often very high, as shown in the interviews. This is so partly because the investments are marginal in various ways and degrees. There is always some old capital waiting to be put to better use inside the firm, and this is also the case in relation to the public sector. This is the interdependence factor. Moreover, the income redistribution effect often seems to be excluded or to be underestimated. Is it not an essential part of the vitality and attractiveness of the capitalistic system that *ex-ante* profitability of investments is expected to be high and promising, while *ex-post* returns, when looking back at past capital costs, seem to be sadly low?

My conclusion from these scattered observations is that interview studies of the type on which I am engaged certainly do not bring much order into the chaos of direct observation. The one good thing about this kind of survey is that it may upset some of the over-simplified assumptions underlying our usual models of growth. In my view, marginal efficiency of investment should be closely related to the *ex-ante* profitability of investment at the various points in the economy and should be determined at given prices and costs. At these points, you may argue, there are micro-elements of Mr. Kaldor's technical progress function. The *ex-post* returns on reproduction costs of capital should not be taken as given parameters in the system of explanation as in the Douglas approach. But obviously the level and changes of *ex-post* profitability of capital, however defined, has to be explained. And one of the factors of explanation should be the flow of investment opportunities with expected high returns in private industry, with a given rate of expansion of the public sector. It may be, as the survey results suggest, that the first measure, that of *ex-ante* profitability in private industry, is often several times higher than the second, the *ex-post* results on an average at the micro points in private industry. But we also have a picture of great dispersion, and when analysing this, account must be taken of the interdependence complications, especially with regard to investment in the public sector. The need for disaggregation follows as a consequence, first and

foremost as between private and public sectors. Within the private sector there should not be complete chaos as to the level and dispersion of profitability expectations.

The third issue I have raised refers to costs of capital. There are capital and credit markets functioning and bringing about some kind of order which have to be studied. The rules of thumb, the rejection rates set up when scrutinising investment projects, the standards of profitability regarded as reasonable in the various firms and branches of activity are not arbitrary and independent of market conditions. The rates of interest on the credit and capital markets, the yields on stocks and the tax rates on net profits have an influence on the rates of net return demanded. In interviews, managers often refer to the costs of new capital as lying between 15 and 20%, and this view apparently determined directly or indirectly the responses to our question of expected returns.

But surely there is hardly a more imperfect market than the capital market. There was a particularly tight rationing of bank credit during the years in question. There are always many more or less water-tight compartments, not least because corporations refuse the market test by retaining their savings. Allocations of savings are distorted in a number of ways, including taxation, weakening any equilibrating mechanism that may exist.

From this point of view, and in the light of the wide dispersion of profitability rates expected from investments being currently made, one conclusion must be this: that more effectively functioning credit and capital markets, with less rationing, more mobility and higher flexibility of capital costs, should make possible a higher rate of increase in productivity.

I am back in the old Swedish habit of facile generalisation and policy recommendation based on very insufficient analysis and factual research. But what can an impatient economist do? I have, on the one hand, been critical of existing models of growth, because of the methods used in treating the essential problems of investment profitability. These methods largely consist in assuming away just those problems that I consider most important if answers are to be given to my economic-policy questions. On the other hand, I have not produced a clear alternative line of approach as a result of my examination of the problems of investment profitability.

I have not succeeded in synthesising all the various aspects of profitability problems that I have identified, but perhaps I have succeeded in giving some reasons why profitability estimates made by experts are interesting as indicators of the level of marginal efficiency of investments, and why return estimates are and must be so difficult to compare. I hope that I have also led you to the conclusion that there is much field work still to be done. If I have at the same time succeeded in disturbing and at best in provoking to new activities some of the skilful model builders among you, some good results may emerge in the longer run.

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THE NEW THEORIES OF THE CONSUMPTION FUNCTION

I. Introduction

There have recently been put forward—notably by Friedman (1957a) and Modigliani and Brumberg (1953 and 1954) [1]—new theories of the consumption function which have startling implications for a wide range of economic problems. It is my belief that the acceptance of these theories has been prejudiced, primarily by the conjunction of valuable and highly controversial hypotheses, and secondarily by certain flaws in exposition. I have, therefore, attempted an exposition of the essentials of these theories which, I hope, avoids these disadvantages.[2]

I distinguish sharply the different hypotheses involved, and consider separately the evidence for, and implications of, each of them. I must apologise to the reader for introducing a new terminology into a subject already overburdened with jargon. My excuse is that I felt it was essential.

II. Notes on Definitions

Initial assets are the individual's net worth at the beginning of the year. A point of minor interest is that, since this includes his holdings of consumer's durables, we are imputing to the individual as income the interest cost of these holdings (except during the current year).

Current income is the total of the individual's receipts (net of tax) during the year—including " earnings " in our sense and the interest yield of assets. The important point here is that contributions by employer or state to schemes for superannuation, health or unemployment insurance are clearly a part of the individual's income and must be included in computations of it.

Earnings differ from income in that (to avoid double-counting) they exclude the interest yield of assets.

Consumption would be perfectly straightforward were it not for the existence of consumers' durables. It has long been recognised (Hicks (1938), p. 176) in economic theory that purchases of consumers' durables are of the nature

[1] The dates are misleading, as the theories were circulated in mimeograph and widely discussed as early as 1953. They had been partly anticipated by Harrod (1948) and Vickrey (1947), but these writers were interested primarily in other problems. The present paper owes a great deal to all three writers, and particularly to personal discussions with them during 1953–54. It has also benefited from the criticisms of Mr. J. S. Cramer and Dr. M. R. Fisher.

[2] The main ways in which my exposition differs from the original theories are as follows. First, I omit the utility theory, which is straightforward mathematics, and not really very illuminating, since uncertainty has to be introduced *after* the results have been derived. Secondly, I avoid Friedman's concept of " permanent income," which, although in some ways very attractive, involves considerable and unnecessary difficulties. In their treatment of the aggregate consumption function the New Theorists diverge sharply, and here I follow Modigliani and Brumberg.

of capital expenditure, whereas the consumption of durables is the appropriate current cost—that is, depreciation plus interest costs. In the early work on the consumption function this was regarded (if at all) as a theoretical nicety, inappropriate to empirical work, and a great deal of the available empirical evidence is based on the assumption that purchases and consumption are equivalent, but in recent years the importance of the distinction has become quite widely appreciated, and not merely among the New Theorists.[1]

Savings are simply the accounting difference between current income and consumption. It should be noted that, as a result of their status as a residual, savings are particularly sensitive to any inaccuracies in the measurement of either income or consumption. In particular, any failure to impute employers' and state contributions to the individual's income is likely to produce a spectacular underestimate of his saving.

We may perhaps add that in *defining* savings as a residual we are not implying anything at all about the motives which lead a consumer to save.

III. The Normal Income Hypothesis

The basis of the new theories is the recognition that, if an individual plans rationally to maximise his utility over his lifetime, his consumption in any given year will depend, not on his income in that year, but on the resources of which he disposes during his lifetime. This is plausible but vague; we must first dispel the vagueness.

If an individual knows with certainty his future stream of earnings,[2] and faces a perfect capital market with a given rate of interest, it is possible to represent " the resources of which he disposes . . . " by the current value v of his current assets plus his expected future earnings, discounted at the rate of interest. The theory could be fully developed in terms of v, but it will assist the exposition if we convert v into an equivalent income stream. Consider a constant annual Y for the remainder of the individual's life-span; if Y is such that the current value of this income-stream is just equal to v, we shall call Y the individual's *normal income*.[3]

It is easy to show that if such an individual also knows his future tastes and the future course of prices, and plans his consumption so as to maximise

[1] For example, Morgan (1951), Goldsmith (1951), Boulding (1950).

[2] " Earnings " is slightly misleading, as we define it to include all receipts except the interest yield of assets. But the obvious alternatives—" income " or " receipts "—would be at least equally misleading.

We must also note that in point of pure theory, earnings are determined by the individual in his maximising calculations, and what is known is the function relating earnings to work done. But this point is not important in the present context.

[3] It should be noted that, given the rate of interest and the life-span, normal income is uniquely determined by v. In this (as well as other ways) it differs from Friedman's " permanent income," which seems a rather flexible concept. We shall argue later that the relevant life-span is not the actuarial expectation of life, but a slightly longer period, not directly measurable. This introduces a (relatively very small) element of flexibility into our concept.

his satisfaction over his lifetime, his planned consumption in each year will be uniquely determined by his normal income Y.

There is not, in practice, a perfect capital market, and this raises a number of theoretical difficulties—notably that it is not clear at what rate to discount future earnings. But these are complications that can easily be dealt with—the main practical difficulty lies in the penal interest rates attached to borrowing that reduces the individual's net worth below a certain level. This means that the above conclusion could not be expected to hold for income-and-consumption patterns that lead temporarily to excessive indebtedness: that is, for income streams that are, in relation to the optimal consumption pattern, unduly deferred. This qualification would probably not be very important, were it not for the problem of durable consumption goods, to which we shall return later.

The fact that we live in an uncertain world is a source of much greater difficulties. Rational behaviour in the face of uncertainty is a problem that has not yet been solved even in simple cases—it would be foolish to suppose that our simple theorem held precisely for so vast a problem as the individual's lifetime consumption plan. But in fact we need only a much more limited result, and one which is quite defensible. Let us now state

> *The Normal Income Hypothesis:* in any given period, an individual's current income y affects his consumption c only through its effect on his normal income Y. We may write $c = \beta(Y)$, where β is independent of current income and assets.

We are only concerned with consumption in the initial period (say, one year) of the plan, and it is reasonable to suppose that the individual's expectations remain stable long enough for him to carry out that part of the plan. Uncertainty about the future will undoubtedly affect his planned consumption in many and complex ways, but we must now consider whether it is likely to give current income any direct influence on current consumption.

One way in which this might happen is that uncertainty about the future might lead people to spend every penny they could lay hands on. Some people probably do this, but not those who have positive savings or assets. This effect seems to be confined to a small minority, and to be unimportant in the community as a whole.

A second possibility is that people's expectations of future earnings should be directly and stably related to their current earnings. This again may be true for a minority, but seems unlikely to be important in the aggregate. More likely is it that, although such a relationship does not hold for individuals, it does hold in the aggregate as a result of averaging out of deviations. This seems quite possible in any particular case, but one would doubt very much the *stability* of such a relationship.

The most important possibility is that uncertainty may lead people to

abandon maximising calculations in favour of conventional rules of the Mrs. Beeton type. This is not unlikely—such rules are important in business behaviour and might be expected to be at least as popular with private individuals. Moreover, they are not necessarily inconsistent with rational behaviour; detailed planning may itself have a disutility, and a conventional rule that approximated the optimal spending pattern might well be most satisfactory to the individual. But just as the business-man adheres to his conventional prices only so long as they are roughly optimal, so we might expect consumers to stick to their conventional saving rule only so long as it gave roughly the right answer. For example, we might expect to find many of those with stable incomes following conventional rules, but we would be surprised if people with very variable incomes did so.

To sum up, for people with variable incomes, the *Normal Income Hypothesis* is roughly equivalent to postulating rational behaviour; but people habituated to stable incomes could quite rationally follow some such rule as "always save $\frac{1}{n}$th of your income"—which would quite invalidate the hypothesis.

IV. The Proportionality Hypothesis

The second hypothesis of the new theories concerns the shape of the function β. They postulate that it shall be a straight line passing through the origin. Formally, this is

> *The Proportionality Hypothesis:* for any individual, the relationship between his consumption and his normal income is one of proportionality.

This was, I think, originally regarded by the New Theorists as purely a working hypothesis, but they may have given the impression that it is an integral part of their theories. At any rate, the attacks on the new theories have centred on this hypothesis.[1] I shall argue, first, that it is unnecessary, and secondly, that it is of doubtful validity.

It is unnecessary because, first and patently, the *Normal Income Hypothesis* is independent of it. More important, we shall see later that the *evidence* for the *Normal Income Hypothesis* is independent of the validity of the *Proportionality Hypothesis*.

I think it is a fair comment on the *a priori* arguments put forward by the New Theorists [2] for the *Proportionality Hypothesis* that they were never designed to do more than establish its plausibility as a working hypothesis; and certainly that they do no more than this. It is also, I think, fair to say (and I shall in part substantiate this comment in a later section) that neither Friedman (1957a) nor Fisher (1956) found any good evidence for the

[1] For example, Friend and Kravis (1957a), Klein and Leviatan (1957).
[2] Modigliani and Brumberg (1954), Part I, Friedman (1957a), Chapter II.

hypothesis. In contrast, Friend and Kravis (1957a) produce a cogent *a priori* argument as to why the proportional relationship should not hold exactly, although it might be argued that the deviations in question affect only the very rich, and are therefore negligible in the aggregate. They also produce (in Table I) the only piece of (more or less) direct evidence that I have yet found. It concerns a sample of families who had had approximately the same income in the previous year and expected much the same income in the subsequent year. The regression of consumption on current income appears to be roughly

$$c = 900 + 0{\cdot}68y$$

There are four qualifications to be made. First, consumption is defined to include purchases of durables; but a similar relation holds for non-durable consumption, and there seems no reason why the case should be different for consumption correctly defined. Secondly, income is defined to exclude the value of employers' and state contributions to schemes for superannuation, health and unemployment insurance. The inclusion of these items would undoubtedly make the results more favourable to the *Proportionality Hypothesis*; but they would have to amount to over $1,300 per family to remove the whole discrepancy. Thirdly, the constancy of current income over three years does not imply logically that normal income must, in the sample, vary proportionately with current income; but I can think of no convincing reason why there should be a substantial and systematic deviation in the requisite direction, except for the possibility that the lower-income groups contain a higher proportion of retired people. Here we must rely on a remark by Friend and Kravis (p. 544) that " when the self-employed and not gainfully employed are eliminated . . . the results are only moderately improved from the viewpoint of the permanent income theories." Fourthly, it is possible that each individual's consumption varies proportionately with his income, but that the factor of proportionality is correlated, within the sample, with income. For instance, intelligent and responsible people might tend to have both higher propensities to save and higher incomes than the rest of the population. Such a correlation would, however, make the Proportionality Hypothesis useless for analysing cross-section data.

In the circumstances, and particularly in view of the hostility provoked by the *Proportionality Hypothesis*, it seems wise to keep an open mind about the shape of the function β.

V. The Short-period Marginal Propensity to Consume

On the *Normal Income Hypothesis* the individual's consumption function is $\beta(Y)$ and his marginal propensity to consume out of normal income simply the slope β' at the appropriate point. We cannot, of course, say anything *a priori* about the magnitude of any individual β', but on the average we would

expect β' to be equal to, or a little less than, unity.[1] But if we wish to calculate the marginal propensity to consume out of current income, we have also to analyse the response of normal income to a change in current income. Formally,

$$\frac{dc}{dy} = \beta' \frac{d\Upsilon}{dy}.$$

We must now separate the direct and indirect effects on Υ of a change in y. The direct effect of an increment Δy in y can be shown to be an increment in Υ of the order of $\frac{2}{N}\Delta y$, where N is the life-span used in the individual's plan.[2] The indirect effect depends on his expectations. Suppose an increase Δy in current income leads him to expect an increase $\mu\Delta y$ in his earnings for n years; then it will produce an increase in normal income of approximately $\frac{n}{N}\mu\Delta y$.

Thus, adding the two effects,

$$\frac{dc}{dy} \simeq \beta' \left(\frac{2}{N} + \frac{n}{N}\mu\right)$$
$$\simeq \frac{2}{N} + \frac{n}{N}\mu$$

Except for those nearing the retiring age, the first term will be small compared with the second, and the individual's marginal propensity to consume out of current income will be dominated by his elasticity of expectations. Table I gives a few illustrative examples, assuming that the individual bases his plans on retiring at 65 and living to 85.

The short-period aggregate marginal propensity to consume is simply a weighted average of all the individual marginal propensities in the community—weighted, of course, by the increments of income received. The individual marginal propensities will vary not only with age, as shown in the table, but also with family responsibilities, social class, and so on. But speaking very roughly, and ignoring possible correlations between these factors and the elasticity of expectations, we may regard the aggregate

[1] If all saving is consumption-spreading and the rate of interest is zero, the individual will plan to spend at some time or other any increment in his normal income. Hence the β''s will average to unity over his life-plan, and thus, too, they will average to unity if the averaging is performed over a "representative" group of individuals. The fact that individuals usually die before the completion of their life-plan (see footnote [2] below) will tend to lower β'.

Saving-for-its-own-sake would tend to lower β', but probably not very much in the aggregate; the effects of a positive rate of interest are difficult to determine (see Graaff (1951)), but are likely to be relatively small.

[2] Consider an income stream $\Delta\Upsilon$ for N years. Its present value, discounted at 5% per annum, falls from $6\Delta\Upsilon$ at $N = 10$ to $10\Delta\Upsilon$ at $N = 40$, so that $\Delta y \simeq \frac{1}{4}N\Delta\Upsilon$.

Since he is uncertain about how long he will live, and since great disutility attaches to exhausting one's assets before one's death, the individual will probably "play safe" by basing his plans on a life-span N considerably greater than his actual expectation of life.

marginal propensity as proportional to the weighted average of individual elasticities of expectations, the factor of proportionality being of the order of 0·5 or 0·6.

This short-period aggregate marginal propensity to consume is, of course, a relationship between hypothetical variations in income and the consequent changes in consumption, for a given group of people in a given year. It is

TABLE I

Age \ μ	0	1	2
25	0·03	0·70	1·37
35	0·04	0·64	1·24
45	0·05	0·55	1·05
55	0·07	0·40	0·73

not strictly applicable to year-to-year changes in income and consumption, as both the composition of the community and the individual β's will, in general, change from year to year. However, in the relatively short period the composition of the community will remain fairly constant and the changes in the β's may be assumed roughly to average out. Another complication is that the level of assets varies in time-series observations, but this can be neglected in the present rough calculations. (The marginal propensity to consume out of assets is of the order of $\frac{2}{N}$—roughly 0·1, allowing for weighting the individual marginal propensities by asset ownership, which is positively correlated with age.)

As the elasticity of expectations is not directly measurable, observations of the cyclical marginal propensity cannot be expected to provide any very satisfactory evidence for or against the hypothesis. However, the readily available data seem to imply an *average* (over time and the community) elasticity of about unity.[1] This cuts both ways. In so far as unity is a plausible value, the data are consistent with the *Normal Income Hypothesis*; but unit elasticity of expectations also suggests the possibility of a simple relationship between y and c, which would make the hypothesis redundant.

It should be emphasised that this section relates only to the *marginal* propensity to consume. The average propensity involves the complicating factors discussed in the next section.

VI. THE LONG-PERIOD CONSUMPTION FUNCTION

It is very tempting to argue that the long-period aggregate marginal propensity to consume is derived from the individual marginal propensities

[1] Friedman (1957a), Table 12. Only lines 6–14 are based on a correct treatment of consumers' durables, and of these only lines 6–9 are for reasonably short periods. These give marginal propensities of 0·72, 0·65, 0·60 and 0·45.

in the same way as the short-period one, save that in the long-period the elasticity of expectations will be greater, thus giving a greater marginal propensity. This is indeed the essence of much of Friedman's treatment of the time-series evidence (Friedman (1957a), Chapter V), but it is, unfortunately, illegitimate. The difficulty is that the composition of the community cannot be assumed constant in the long period; and further, that the changes in composition found in real life have a considerable effect on the consumption function.

If all saving is for the purpose of consumption-spreading, and if there is no certainty, each individual will, over his lifetime, exactly consume his total income—that is, his savings would sum to zero over his lifetime. In a community which was in all relevant respects stationary, this would imply that individual savings summed over the community would be zero in any year. On these assumptions, positive savings in an economy would be evidence that it was *not* stationary. If dis-saving is principally done after retirement, there are two obvious forms of change that can lead to positive aggregate savings.

First, a growing population will, in general, mean a disproportionately small number of retired people, and hence of dis-savers. Secondly, a rising level of real income per head will mean that the savers will, on the average, have larger lifetime incomes than the dis-savers. Modigliani and Brumberg (1953), in a most striking piece of research, investigated these effects in detail. They found that, on a number of simplifying assumptions, for rates of growth up to 5% per annum, each 1% per annum of growth in either real income per head or population would lead to 3–4% of aggregate income being saved. Thus the saving-ratio is proportional to the rate-of-growth of aggregate real income, and is independent of how this growth is compounded of changes in population and in real income per head (so long as both change steadily). This suggests

> *The Rate-of-growth Hypothesis:* In long-run equilibrium, aggregate saving is determined by changes in population structure and in real income per head. If these factors change steadily, the fraction of aggregate income saved is proportional to the rate of growth of aggregate real income.

The simplifying assumptions include a zero rate of interest, consumption spread evenly over the life-span and no bequests. There seems to be no reason why the calculations should be very sensitive to a positive rate of interest, or to plausible variations in the consumption pattern—so long as lifetime consumption equals lifetime income. But the assumption of no bequests seems crucial—and palpably false.

However, the hypothesis would still be valid if heirs planned to dis-save (the bulk of) their inheritances over their lifetime. They would do this on the *Normal Income Hypothesis* provided that saving-for-its-own-sake or for

posterity was small; but they would also do so if they were " improvident," *i.e.*, deviated from the life-plan in the direction of shorter-run maximisation of utility. They would fail to dis-save adequately if for some reason they wished to maintain or increase their assets, or if they adopted some conventional rule (such as " never spend out of capital ") which led them to do so. Such behaviour we may call " thrifty." The building-up of family fortunes over several generations is evidence of thrift, but the nineteenth-century saying, " From clogs to clogs in three generations," shows that even in the golden age of thrift, dis-saving by heirs was not uncommon.

It is obviously important to find out whether thrift generates a significant volume of saving, and Goldsmith (1955) gives estimates [1] of asset holdings by different age-groups which permit us to make a rough test. If the population is assumed to decline linearly after the age of 65 and if individuals are supposed to plan a linear running down of their assets over the same number of years, it is easy to show that average assets in the over-65 age-group will be $\frac{2}{3}$ of those at 65, assuming a history of stationary prices and incomes.

Goldsmith's Table XLII, Col. 1, shows observed net worth by age-groups, and estimating the 65 value by extrapolation, we get a ratio of 0·73. Goldsmith suggests (Vol. I, p. 217 n.) that this figure is too high, owing to the oldest age-group's having had a particularly favourable history of capital gains. In Col. 3 of the same table, Goldsmith gives estimates of accumulated life saving which yield a ratio of 0·52. (He regards these estimates as giving " the least distorted picture of life saving curves that can be fashioned out of the rough over-all data now available.") Unfortunately, the asset figures underlying both estimates exclude consumers' durables other than motor cars, and so probably give a downward bias; but this should not be large.

These calculations suggest that the population as a whole is not thrifty.[2] However, figures based on estate-tax data (Vol. III, Table E-29) give ratios of approximately unity, which are presumably underestimates, as no allowance is made for gifts *inter vivos*. Thus, thrift is important among those subject to estate tax, and its absence from the aggregate results is due to the cancelling out of thrift among the wealthy and improvidence among the poor.

The direct evidence of Modigliani and Brumberg's calculations leads to much the same conclusion. Saving 12% of aggregate income would, on the *Rate-of-growth Hypothesis*, correspond to aggregate real income growing at 3–4% per annum. Thus the saving that would be generated by growth on the hypothesis is of approximately the same amount as that observed; but this could easily conceal substantial cancelling of errors, and in particular

[1] Vol. I, pp. 214–25; Vol. III, pp. 102–35 and 284–381.

[2] The data are approximate, and the theoretical figure of $\frac{2}{3}$ depends on linearity assumptions that may well not hold exactly. We must therefore allow for sizeable possible errors. Comparing an observed 0·52 with a theoretical 0·67, it seems reasonable to rule out the possibility that the observed value is really greater than the theoretical, but it would be risky to make any more precise inference.

the balancing of thrift among the wealthy against improvidence among the poor.

We may conclude that the *Rate-of-growth Hypothesis* gives a good explanation of the long-period consumption function of the United States, but that this success depends partly on a balance of thrift and improvidence in different normal income groups. In applying the hypothesis to other economies, we must be prepared for the possibility that the balance will not hold there so that actual saving will differ from that predicted by the hypothesis. But however much thrift or improvidence may distort the picture, the rate of growth will remain the basic determinant of the aggregate savings/income ratio in the long run.

It is perhaps worth noting that the evidence considered in this section is quite unrelated to the *Normal Income Hypothesis*. Nor do Modigliani and Brumberg's calculations throw any light on the *Proportionality Hypothesis*—it is virtually impossible to make inferences from the observed long-run aggregate consumption function about the shape of the individual consumption functions. On the other hand, Goldsmith's data constitute some evidence against the *Proportionality Hypothesis*.

VII. The Friedman Effect

If consumption depends on normal income, the interpretation of a sample survey which gives observations of consumption and current income must depend on the relationship in that sample between current and normal income. Friedman (1957a, Chapter III) and Modigliani and Brumberg (1954, Part II) give *a priori* arguments that there will usually be a particular relationship between these quantities. We shall now attempt a brief summary of this argument. The brevity is achieved by sacrificing some detail and rigour, and also (I regret to say) by introducing a new notation.

Suppose that for a sample of families the elasticity of consumption with respect to normal income is E, and that the apparent elasticity with respect to current income, computed from survey data, is G. Suppose, too, that we can measure the relationship between normal income Υ and current income y by the elasticity F of Υ with respect to y. Then we can see at the intuitive level that $G = EF$, so that, for instance, if Υ increases proportionately with y (that is, if $F = 1$) G is an estimate of E.

It is argued by the New Theorists that F is usually less than unity. Suppose that for the families in question year-to-year fluctuations in current income *which do not affect normal income* are important. Suppose further that in the particular period to which the survey refers the magnitude and direction of these fluctuations is not correlated with normal income. (This would clearly be true if we were considering the average over a number of years, but any particular year might, for example, be a good one for the rich and a bad one for the poor.) Then, abstracting from possible complications through the correlation of normal income with age, the magnitude and

direction of the temporary fluctuations will be positively correlated with current income. In other words, in the higher current income groups a larger proportion will be enjoying a "good year." This implies that Υ increases less than proportionately with y, and thus that $F < 1$.

We shall find it convenient to call this phenomenon, of $F < 1$, the "Friedman Effect," and to speak of a smaller value of F as a larger Friedman Effect. The argument for the Friedman Effect is plausible, but equally it is clear that it may apply to some societies or social groups and not to others. As normal income is not directly measurable, we can make no direct tests, but there is a certain amount of indirect evidence.

Table II, for example, shows for various American occupation groups the

TABLE II

	A	B	C
Professional	1–5	61	60
	6–8	52	70
	9–10	69	53
Self-employed or managerial	1–5	23	35
	6–8	38	51
	9–10	50	50
Clerical and sales	1–3	65	68
	4–5	60	59
	6–8	62	58
	9–10	61	63
Skilled or semi-skilled	1–3	40	50
	4–5	48	49
	6–8	63	54
	9–10	68	63
Unskilled	1–3	38	47
	4–5	43	53
	6–10	55	68
Farm operator	1–3	26	40
	4–5	47	48
	6–10	58	53

A. Income group.
B. Percentage of those in that income group for 1947 who had an (appreciably) larger income than in 1946.
C. Percentage of those in that income group for 1948 who had an (appreciably) larger income than in 1947.
Data abstracted from Katona and Fisher (1951), Table 8.

percentage of families in each current income group who were better off than they had been in the previous year. Where the Friedman Effect is present, this percentage will increase with income. Such a tendency is well marked among farm operators, manual workers and the self-employed and managerial groups. There is clearly no such effect in the "clerical and sales" group, while the professional group shows unsystematic movements.

Some similar figures for Dutch families are given in Table III. The occupation groups are *handarbeiders* (manual workers) and *hoofdarbeiders* (roughly, clerical workers). For each occupation the average current income of each current-income group is compared with its average income

over the previous four years. If the latter is taken as a measure of normal income the manual workers display the Friedman Effect while the clerical workers do not.

TABLE III

Income group Fl. per annum	*Handarbeiders*			*Hoofdarbeiders*		
	No. of families	Average current income	Average previous income	No. of families	Average current income	Average previous income
<1,600	72	1,144	1,183	34	1,136	1,110
1,600–2,000	33	1,740	1,623	21	1,745	1,672
2,000–2,600	18	2,215	2,073	36	2,287	2,151
2,600–3,400	8	2,950	2,460	31	2,983	2,739
3,400–4,500	1	4,140	3,855	22	3,847	3,503
4,500–6,700	1	4,832	4,111	12	5,094	5,034
>6,700	—	—	—	21	22,489	26,485

Taken from: "Resultaten von een inventarisonderzoeh in de Gemeente Rotterdam, ingesteld door de Schade Enquête Commissie aldaar, in 1941" in *Statistische en Econometrische Ondetzoekingen*, Maandschrift van het Centraal Bureau v.d. Statistiek 1943, nr. 4.

Mr. J. S. Cramer comments: "Not a very good sample, 'selected' by asking firms to ask for volunteers from among their employees." He quotes, "In several cases the current income could be ascertained with more accuracy than the 5 year average."

This evidence, limited though it is, is enough to confirm our impression that the Friedman Effect may well be present in samples from some occupation groups—such as farmers, business-men and manual workers—and missing from other samples, such as professional men and salaried workers.

VIII. The Evidence from the Friedman Effect

Friedman's analysis of the cross-section evidence for his theory (Friedman (1957a), Chapters IV, VI and VII) is a most brilliant and fascinating economic argument, and occupies 132 pages. The present short discussion cannot do justice to the rich detail of his analysis, but it will, I hope, serve to indicate both the strengths and the weaknesses of his arguments.

His analysis of the survey data is based on three assumptions:

(*a*) that $F < 1$, *i.e.*, that the Friedman Effect is present;
(*b*) the *Normal Income Hypothesis*;
(*c*) that $E = 1$, *i.e.*, the *Proportionality Hypothesis*.

He finds almost all the evidence he considers consistent with this set of assumptions. Unfortunately, since direct measurement of F is impossible, it is impossible to estimate accurately the value of F for any particular sample, so that this "consistency" does not go very far towards confirming the hypotheses.

But although the absolute magnitude of F is difficult to estimate, there may be good grounds for believing that F will be smaller in one group than

another, and this will provide a test of the *Normal Income Hypothesis*. For suppose we have reason to believe that F is smaller in group A than in group B, and no reason to expect a significant difference in the values of E. Then we must expect to find a smaller G in group A on the *Normal Income Hypothesis*, and not otherwise. (Once again, the test is independent of the *Proportionality Hypothesis*.)

Friedman quotes a number of pieces of evidence of this type. In Table I, for example, he gives the values of G computed from a number of surveys in the United States, Britain and Sweden. One would expect that, of these three countries, year-to-year fluctuations would be most important in the United States and least so in Sweden. This accords with the values of G, which are systematically larger in Sweden than in the United States, with Britain in an intermediate position.

It is a commonplace of economics that farm incomes are particularly subject to year-to-year fluctuations, and much the same is true of the business community. Friedman shows (Tables 1, 3 and 4) that United States farm families exhibit smaller values of G than the rest of the community. In Table 5 he gives the results of a survey in which families are classified as "independent business," "farmers" and "others." The two former groups show values of G that are almost equal and much smaller that that of the "other." On the other hand, in Table 6 the G for independent businessmen is higher than for most other occupations; but Friedman attributes this to a faulty definition.

Fisher (1956) gives similar evidence from a British survey. He gives values of G (the figures called P_γ in columns (b) of Table 2.5) for four occupation groups—manual, clerical and sales, managerial and self-employed—broken down by age. If all ages are taken together, the self-employed have the lowest G, but this does not hold for all the individual age-groups. The number of observations in some of the cells is quite small, but even so, this unsystematic result may be a sign of the importance of factors that vary with age.

Perhaps a more important aspect of Fisher's results is that the manual workers have an unusually high G. In view of the low value of F in Table II above, this must suggest a breakdown of the *Normal Income Hypothesis*. Of course, the figures in Table II were for American workers, and their British counterparts may well have a much higher F; but, as we shall see later, this in itself suggests a limitation of the hypothesis.

Friedman bases a further test on the argument that classifying a group of families according to the difference between their current income and that of the previous (or subsequent) year will increase F. The results he quotes in Tables 10 (farm families) and 11 (mainly urban) appear to show that such classification produces a definite reduction in G.

In those rare surveys that record the family's income for two consecutive years, it is possible to make rather more direct tests. Friedman develops a

method of estimating F for a group of families from the regression of their current on their previous year's income (or vice versa). I am not convinced that F is more closely related to these estimates than to the correlation coefficient of the two years' incomes, but it is clear that all four quantities are likely to be fairly highly correlated.

In Table 20 Friedman gives these statistical measures and values of G for three occupational groups—independent non-farm business, farm operators, and clerical and sales. Although one should not attach undue significance to a correlation based on three observations, it is noteworthy that the values of G (line 4) are virtually identical with those of the correlation coefficient (line 5).

A more substantial piece of evidence of the same sort is quoted by Friedman from an unpublished paper by Margaret Reid. In Fig. 15, Panel 1, he shows estimates of F, similar to those discussed in the previous paragraph, plotted against values of G for 27 groups of families. The correlation is most impressive, although it must be said that all the groups but one are farm families, and that it is not clear whether all the points plotted represent different groups, as opposed to the same group at different times. There is also the possibility that these account-keeping families might be unusually sophisticated in their economic thinking, and perhaps therefore obey the *Normal Income Hypothesis* where others would not.

In any test where one uses the previous year's income as a proxy for normal income, it is always possible that the effects one attributes to the *Normal Income Hypothesis* are really due to a simple lag in the adjustment of consumption to income. This is, of course, not so where the *subsequent* year's income is the proxy, but it is fortunate that Friedman provides a further piece of evidence on this point. In Fig. 15, Panel 2, he again plots estimates of F against G, but here the estimates use the *penultimate* year's income as a proxy. Most forms of adjustment lag would imply a substantially lower correlation for Panel 2 than Panel 1, while the *Normal Income Hypothesis* suggests a roughly equal correlation. On inspection there appears to be a slight reduction, which may be partly explained by the fact that these calculations were possible for only some of the groups in Panel 1.

Individually, each of the pieces of evidence described above lacks the conclusiveness of a direct test. But together, I feel, they constitute a fairly strong confirmation that the effects predicted by the *Normal Income Hypothesis* occur in real life. However, we must be careful not to overstate the case, for the fact that some people behave in a certain way does not mean that all do. In fact, if we look at the data with a view to determining what sort of people have gone into our tests, we find that the tests fall into three groups:

(*a*) tests based solely on figures for farmers and/or business-men;

(*b*) tests based on contrasting the behaviour of farmers and business-men with that of the rest of the community; and

(*c*) tests based on samples for the whole community, in which farmers and/or business-men may be sufficiently important to account for the results we found.

Thus the evidence shows only that farmers and business-men behave according to the *Normal Income Hypothesis*—it says nothing about the behaviour of the rest of the community. Now these two groups are those for whom year-to-year fluctuations in income are particularly important, and for whom therefore conventional saving rules are most obviously irrational. For a man with a secure job at a steady salary, on the other hand, a conventional rule might work very well. We must therefore be prepared to find that those with steady incomes do not obey the *Normal Income Hypothesis*.

We are not, then, surprised to find a negative result in the case of the British manual worker, habituated to over-full employment. The same might turn out to be true of clerical workers, who do not display the Friedman Effect either in Holland or the United States, but, on the other hand, American manual workers, a large group who show a substantial Friedman Effect, might prove to obey the hypothesis. However, this is conjecture. The fact is that there is strong evidence that the hypothesis holds for farmers and business-men, and little or no evidence on the rest of the community.

We may note finally that since all the tests depend on relative magnitudes of F, their validity is independent of the *Proportionality Hypothesis*; and in so far as we have not felt able to provide estimates of the absolute magnitude of F, we have been unable to test the *Proportionality Hypothesis*.

IX. Some Further Considerations

Mr. H. W. Watts (1958, pp. 126–32) has recently produced evidence of a rather different sort for the *Normal Income Hypothesis*. He argues that a measure of the expected future income of a young man is given by the " cross-section income profile " of men with the same occupation, education and so on. (The " profile " is obtained by plotting average income against age for suitably qualified members of a cross-section survey.) He compares groups of men with different educational backgrounds, but otherwise similar, and finds that, for a given current income, the better educated have higher expected incomes and higher current spending.

The possible effects of education on spending behaviour are complex. Watts argues that it would promote rational rather than improvident behaviour, and thus lead, *ceteris paribus*, to higher saving; but it would also promote rational rather than thrifty behaviour. In addition, it might be argued that education develops a taste for the good things of life—books, foreign travel, winter sports, wine and food, and so on—and that this is only partly offset by the gambling and the television sets of the uneducated. However, Mr. Watts' results, so far as they go, favour the *Normal Income Hypothesis*.

"Contractual saving"[1]—for example, saving in the form of life-assurance premiums or annuity or mortgage payments—is a common phenomenon nowadays and is likely to work against the *Normal Income Hypothesis*. It is likely to make it more difficult for a man to reduce his net saving as much as would be rational during a temporary fall in income. However, one suspects that its popularity is largely confined to people with stable incomes—those with variable incomes are likely to have any contractual saving well-cushioned with liquid assets.

We have said little about saving for posterity. My own belief is that, while many parents give generously to their children during their own lifetime (and while the children are relatively young), few would deliberately reduce their own consumption in order to make a *post-mortem* gift to children who would by then be middle-aged. Bequests are certainly made, but these would be due to uncertainty as to the life-span, or to the need to maintain an equity in utility-yielding assets. However, it is possible that some people save in order to leave money to their children, and we must ask how this would affect our various hypotheses.

The phenomenon can best be represented by making the functions $\beta(y)$ lower and less steep, on the average over a man's life, than they would otherwise have been. This will not affect the *Normal Income Hypothesis*, but will constitute an additional reason for doubting the *Proportionality Hypothesis*. So far as the *Rate-of-growth Hypothesis* is concerned, it will be just another factor making for thriftiness.

I am indebted to Mr. N. Kaldor for pointing out that, whereas capital gains are assumed by the New Theories to be included in current income, they are not so included in most of the data we have used. The cross-section data are the most important, and it is easy to show that here the exclusion of capital gains leads to over-estimates of both the marginal and average propensities to consume. However, we have worked in terms of the income-elasticity of consumption, and the effect here depends on the relative magnitude of these two biases. In fact, if in any sample capital gains, on the average, increase more than proportionately with income—as I think we would expect—then the income-elasticity of consumption will be over-estimated. This tends to strengthen the evidence against the *Proportionality Hypothesis*, but its implications for the *Normal Income Hypothesis* are less clear. Our evidence there consisted in comparisons of income-elasticities for different groups, and would therefore be affected only by differential bias between groups—a subject on which *a priori* argument is rather hazardous, though one would expect such differential effects to be relatively small.

The omission of capital gains from aggregate time-series of personal disposable income will lead to an over-estimate of the short-period marginal propensity to consume if, as seems likely, capital gains are positively correlated with income. This means that the implied values for the elasticity

[1] See, for example, Klein (1954).

of expectations, towards the end of Section V, will be somewhat too high; but it is doubtful whether this will make an appreciable difference to such rough calculations.

X. Conclusions

We have broken the New Theories down into three independent hypotheses. Of these, we have left on one side the *Proportionality Hypothesis*; it is unnecessary to the theories, and such evidence as we have found is against rather than for it. On the other hand, we found the *Rate-of-growth Hypothesis* to be substantially valid. Certainly, in the long-run the level of aggregate net saving is determined by balancing gross saving against dis-saving, and is therefore sensitive to changes in the structure of the economy that affect this balance. But it is also determined by the balancing of thrift and improvidence, and although in the United States these factors roughly cancel each other out, in other economies the balance might be different, with a consequent difference in the savings function.

It is thus an over-simplification to assert that, in the long-run, the proportion of aggregate income saved is proportional to the rate of growth of aggregate income; but it is much nearer the truth than the linear consumption functions so often postulated. It would be interesting to see the effect on the many " theories of economic growth " of substituting in them the *Rate-of-growth Hypothesis* for their present (usually linear) consumption functions.

Once the importance of structural factors is recognised, a vast new field of useful, scientific economic research is opened up. The effects of variations in the birth-rate, of emigration and of changes in the rate of change of productivity are all of great practical importance and open to relatively easy investigation.

The *Normal Income Hypothesis* we found to be well-substantiated for farmers and business-men in the United States, but we kept an open mind as to how far it extends to other countries or occupations where incomes are more stable. However, even if the hypothesis were valid *only* for these groups (and there is no evidence for this) its importance is established. Friend and Kravis (1957b) estimate (p. 296) that these groups are responsible for 55–65% of personal saving in the United States, and (p. 278) that their marginal propensity to save is more than twice that of the rest of the community. As it is well known that their incomes are more variable, it is clear that their behaviour determines a very large proportion of the variation in personal savings.

This serves to put trade-cycle theory on what one might call " a fully expectational footing." The recent orthodoxy has been an expectational theory of investment *plus* a mechanistic consumption function—now consumers can have their expectations too! We must, however, enter a caution. Employment and trade-cycle theory depend, not on the marginal propensity to consume but on the marginal propensity to *spend*, which may be very much

higher. A consumer may well use a temporary increase in his income to purchase some item of durable equipment, thus spending a large part while consuming only a small fraction of the increase. Another way of putting this might be to say that consumers' durables introduce a sort of accelerator effect on the consumer side.

The hypothesis also has implications for the use of budget studies to throw light on consumption behaviour. We have seen that where the Friedman Effect is present and the *Normal Income Hypothesis* holds, a simple regression analysis will give a biased estimate of the income elasticity of consumption. If a partial regression analysis of, say, consumption on current income and a third variable x is made in the same circumstances, even more misleading results can be produced. For a linear function of x and current income may well be significantly more closely correlated with normal income than is current income alone. In this case, x will appear to have a significant effect on consumption, even though there is no causal relation at all. Friedman (1957a, pp. 86–90) gives an excellent discussion of such possibilities.

It is clear that these phenomena make the interpretation of cross-section studies where only current income is recorded extremely hazardous. So far as existing data are concerned, it behoves the research worker to tread warily, with an eye constantly turned towards the possible effects of Normal Income. For future surveys, one hopes that, at the very least, income for several previous years and expected future income will be recorded; it is perhaps too much to hope that cross-section surveys will be replaced by the much more informative continuous budget studies.

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OPEN-MARKET OPERATIONS *VERSUS* RESERVE-REQUIREMENT VARIATION[1]

I

WIDELY divergent conclusions have been reached in discussions of the proper choice between open-market operations, on the one hand, and variation of cash-reserve requirements, on the other. Whereas several academic economists have suggested increases of reserve ratios as an alternative or supplement to counter-inflationary open-market operations, central banking authorities, as well as commercial banking circles, are generally opposed to such an innovation.

Thus, in comparing the short-period application of the two instruments, Professor R. S. Sayers considers it a " fact that, if restriction of the deposit total [of commercial banks to a particular level] is thought right by the central bank, its enforcement by one weapon is merely a substitute for enforcement by another." [2] Accordingly, he adjudges as lacking in substance " the bankers' dislike of the reserve variation weapon . . . when this weapon is confined to short-period service as an alternative to open-market operations." [3] His reason for this view is that, " on the assumption that the central bank achieves equal success in its attempts to regulate the liquidity of the economy, the profitability of banking is unaffected by the choice between variation of reserve ratios and open-market operations." [4]

By contrast, the Board of Governors of the United States Federal Reserve System states that:

> " The reserve requirement instrument is not . . . interchangeable with the open-market instrument. Unlike open-market operations, the results affect immediately and simultaneously all banks. . . . Changes in requirements, moreover, cannot be made frequently—especially on the up side—without unduly disturbing the operations of individual banks. . . . Changes in reserve requirements are, therefore, made infrequently and typically involve a fairly sizable volume of funds. The effects tend to be large and concentrated within a short period of time. The instrument is more appropriate for making a major change in the volume of available bank reserves than it is for short-run adjustments. It is not adaptable to affecting bank reserve positions on a day-to-day and week-to-week basis, as are open-market operations. In fact, it may be desirable to engage in partially offsetting open-market

[1] I am indebted to Professors G. H. Evans, Jr., F. Machlup, A. T. Peacock and R. S. Sayers for some very helpful suggestions.

[2] R. S. Sayers, *Central Banking After Bagehot* (Oxford, 1957), p. 89.

[3] *Ibid.*, p. 90.

[4] *Ibid.*, p. 89.

actions in order to cushion the impact of reserve requirement changes in credit markets." [1]

In general accord with Professor Sayers' reasoning, other academic economists have recurrently argued that changes in reserve requirements could be made by much smaller degrees than hitherto, correspondingly reducing the resultant shock effects.[2] Furthermore, it has been suggested that the immediate and simultaneous impact of a given change in reserve requirements upon all commercial banks may be advantageous by comparison with the delayed effects of open-market operations upon banks removed from the money-market center.[3] Finally, it has been pointed out that the direct price and yield effects on the government securities market would be more moderate for a given increase of reserve requirements than for a comparable magnitude of open-market operations.[3]

Member-bank Reserve Requirements in the United States, January 1951–June 1959
(Per cent of deposits)

Effective date of change.	Demand deposits.			Time deposits.	
	Central reserve. City banks.	Reserve. City banks.	Country banks.	Central reserve and reserve. City banks.	Country banks.
1951—Jan. 11, 16 *	23	19	13	6	6
Jan. 25, Feb. 1 *	24	20	14	—	—
1953—July 1, 9 *	22	19	13	—	—
1954—June 16, 24 *	21	—	—	5	5
July 29, Aug. 1 *	20	18	12	—	—
1958—Feb. 27, Mar. 1 *	19½	17½	11½	—	—
Mar. 20, Apr. 1 *	19	17	11	—	—
Apr. 17	18½	—	—	—	—
Apr. 24	18	16½	—	—	—
In effect June 1, 1959	18	16½	11	5	5

* First-of-month or mid-month dates are changes at country banks, and other dates are at central reserve city or reserve city banks.

SOURCE: *Federal Reserve Bulletin*, Vol. 45 (June 1959), p. 600.

Renewed suggestions that variable reserve ratios be adopted as an additional counter-inflationary weapon by the Mother of central banks[4] contrast with refusal of the American monetary authorities, already endowed

[1] Replies of the Chairman of the Board of Governors of the Federal Reserve System to Questions Submitted by the Subcommittee on Economic Stabilisation of the Joint Committee on the Economic Report, in volume of Hearings on *United States Monetary Policy: Recent Thinking and Experience* (Washington: December 6 and 7, 1954), pp. 11–12.

[2] See Charles R. Whittlesey, "Reserve Requirements and the Integration of Credit Policies," *Quarterly Journal of Economics*, Vol. LVII (August 1944), pp. 558–64.

[3] See Ira O. Scott, Jr., "The Regional Impact of Monetary Policy," *Quarterly Journal of Economics*, Vol. LXIX (May 1955), p. 283.

[4] See Sayers, *op. cit.*, p. 91, and his reference (p. 85) to Sir Theodore Gregory, *The Present Position of Central Banks*, 1955 Stamp Memorial Lecture (London, 1955), p. 21.

with this tool for more than two decades, to apply it restrictively ever since March 1951. As shown in the table on p. 698, all changes of member-bank reserve requirements in the United States since March 1951 to the time of this writing have been in but one direction: downward. Hence, a reconsideration of reserve-requirement variation in comparison with open-market operations appears timely, both on theoretical and on policy grounds.

In line with the literature on the subject, the next three sections will assume a closed economy. In the final section this assumption will be dropped.

II

Before analysing the differential impact of open-market operations versus that of variation of reserve requirements, it is useful to specify the economic situation in which central-bank action is assumed to take place. Three types of situation may be distinguished.

In the first type of situation commercial banks are without significant excess reserves, and are attempting to switch out of government securities into private loans because of a boom in the demand for credit. This would be the state of affairs associated with a restrictive monetary policy.

In the second type of situation, too, commercial banks are without significant excess reserves but in equilibrium with respect to the structure of their assets. This would be the state of affairs associated with a " neutral " monetary policy.

In the third type of situation commercial banks have considerable or substantial excess reserves. This would be the state of affairs associated with an expansionary monetary policy. Let us take up these three types of situation in turn.

(*a*) Suppose that, being confronted with the first type of situation, the central bank has decided to reduce the volume of member bank reserves by a given amount, and is considering whether to do so by open-market sale of government securities or by raising member-bank reserve requirements. In this case, is it correct to say that there is no substantive difference between the effects of the two courses of action? Certainly not.

In the face of a boom in the demand for private credit the proximate objective of a restrictive monetary policy is to curb the switching by banks from government securities into commercial loans. Yet the magnitude of such switching would be greater in response to newly raised reserve requirements than it would be in response to an equivalent sale of government securities by the central bank. This outcome is due to two factors.

In the first place, the imposition of higher cash-reserve requirements reduces to zero the rate of return on a certain proportion of commercial banks' assets that have hitherto yielded a positive rate of return. In other words, the higher reserve requirements reduce not only the total volume of commercial-bank assets but also the proportion of commercial-bank earning assets to their total assets. On the other hand, open-market operations

reduce the total volume of commercial-bank assets without reducing the proportion of earning assets to total assets. Thus imposition of the higher reserve requirements has a more restrictive impact on commercial-bank income than the equivalent volume of open-market sales by the monetary authority. Therefore, the marginal utility of bank income is higher as a result of increased reserve requirements than as a result of open-market sales by the monetary authority.[1] The higher marginal utility of bank income induces commercial banks to sacrifice liquidity, *i.e.*, to shift further out of government securities into loans, in order to obtain additional earnings.

Secondly, the liquidity needs of commercial banks are to a greater extent met by the required reserves themselves with a higher reserve requirement than with an unchanged reserve requirement. To illustrate, if required reserves are 10%, and bank deposits drop from 100 to 80, only 2 units of the 20 cash are available for paying off the deposits. On the other hand, if required reserves are 20%, and deposits drop from 100 to 80, 4 units of the 20 cash are available for paying off the deposits. Thus the higher reserve requirements reduce the marginal utility of the moneyness of bank assets, also inducing commercial banks to sacrifice liquidity in order to obtain additional earnings.

Under conditions of a restrictive monetary policy, *i.e.*, amid excess demand in the market for private credit, banks are afforded ample opportunity to offset the constraint placed upon them by the higher reserve requirements. The "income" effect and the "liquidity" effect of imposition of the requirements jointly induce banks to switch out of governments into loans on a larger scale than in response to open-market sales on the part of the monetary authority.

(*b*) Our foregoing analysis of the first type of situation applies with but minor modification to the second type of situation. Here the "income" effect and the "liquidity" effect of an increase in reserve requirements will still take place. But since there is, by assumption, no excess demand for private credit, commercial-bank shifting from governments to loans will be smaller than in the first type of situation. In other words, the impact of imposition of higher reserve requirements will not be as perverse if, to begin with, commercial banks were in an equilibrium position with respect to the structure of their assets.

(*c*) Though the third type of situation—widespread excess reserves in commercial banks—is commonly associated with an *expansionary* monetary policy, let us for the moment consider the implications of *higher* reserve requirements in this context.[2] Here the imposition of higher reserve requirements produces neither the "income" effect nor the "liquidity" effect so long as the pre-existing excess cash reserves fully meet the higher reserve

[1] Alternatively stated, the marginal rate of substitution of bank income for bank liquidity will be lower as a result of the rise in reserve requirements than as a result of open-market sales by the monetary authority.

[2] The relevance of this consideration to the real world will be discussed in Section IV.

requirements. For so long as the higher reserve requirements can be satisfied without liquidation of earning assets, their imposition does not adversely affect the present income position of the banks involved; nor does it imply an increase in the proportion of liquid assets to total bank assets.

Indeed, amid widespread excess reserves, imposition of higher reserve requirements is, from the viewpoint of debt management, preferable to open-market sale of government securities by the central bank. This is so because higher reserve requirements that merely absorb pre-existing excess reserves do not necessitate any reduction in the government-security holdings of commercial banks and, hence, do not entail a rise in interest rates on government securities. On the other hand, the open-market operation of the central bank adds to the supply of government securities in the open market, a factor making for higher interest rates.

III

The upshot of the preceding section is that there exists an asymmetry between imposition of higher cash-reserve requirements and open-market sale of government securities by the central bank. Specifically, the following conclusions emerge:

(1) Amid booming demand for credit, concomitant with a widespread absence of excess cash reserves in commercial banks, higher reserve requirements will induce more shifting by banks from government securities to private loans than would restrictive open-market operations. This, in turn, implies that effective interest rates on government securities will rise to a greater extent in response to higher reserve requirements than in response to restrictive open-market operations.

(2) In the absence of buoyant demand for credit and in the absence of widespread excess reserves that could meet higher cash-reserve requirements, the higher requirements would still induce more shifting by banks from governments to loans than would restrictive open-market operations. This asymmetry, however, would not be as pronounced as under the conditions of conclusion (1). It still follows that interest rates on government securities will rise to a greater extent in response to higher reserve requirements than in response to restrictive open-market operations; though, again, the asymmetry will be less significant than under the conditions of conclusion (1).

(3) Amid widepread excess reserves that will fully satisfy higher cash reserve requirements, the imposition of higher requirements avoids a rise in interest rates on government securities, since bank shifting from governments to private debt is not made necessary. On the other hand, restrictive open-market operations will raise interest rates on government securities because they augment the supply of governments to the private economy.

Manifestly, conclusions (1) and (2) must be qualified to the extent that there are differences in the reserve positions of different commercial banks. Likewise, conclusion (3) must be qualified to the extent that all or some

banks lack excess cash reserves to fulfil the higher reserve requirements and are, therefore, compelled to liquidate earning assets.

IV

We may now attempt to reassess the running debate between various academic economists and the Federal Reserve on the comparative advantage of changes in reserve requirements versus open-market operations.

To recall, it has been suggested, particularly in the United States and largely in criticism of Federal Reserve policy, that increased use of changes in cash-reserve requirements is preferable to heavy reliance on open-market operations. This suggestion has been justified on two distinct grounds. First and most frequently, it has been asserted that restrictive open-market operations involve direct price and yield effects in the government-securities market which could be avoided by more extensive use of changes in reserve requirements. Second, it has been contended that transmission of the impact of monetary policy throughout the commercial banking system could be accomplished with shorter time lags by means of variation of reserve requirements than through open-market operations. To the oft-repeated Federal Reserve argument that changes in reserve requirements are too blunt an instrument, the repeated counter-argument has been that "The large and infrequent changes in reserve ratios, which the Federal Reserve takes to be the result of defects in the instrument, may instead be responsible for those defects. Open-market operations of comparable magnitude can also be a shock to the markets." [1]

What implications do our conclusions have for the opposing views in this continuing controversy? Is there really no more to the Federal Reserve's rejection of the academicians' argument than a tendency "toward an immaculate, high-church, and 19th century view of [central-bank] responsibilities"? [2]

Our first and second conclusions sharply reduce the validity of the argument that changes in cash-reserve requirements will shield the government-securities market from the price and yield effects entailed by open-market operations. As implied by our third conclusion, this argument is applicable only to the type of situation wherein commercial banks possess sufficient excess cash reserves to meet higher reserve requirements without having to resort to the liquidation of earning assets. (This, indeed, is the type of situation adduced by Professor Sayers in his denial of an asymmetry between the two central banking instruments.[3]) And this kind of situation is rarely one that is associated with the choice between higher reserve requirements and restrictive open-market operations: rather, it is the kind

[1] Edward S. Shaw in *United States Monetary Policy: Recent Thinking and Experience*, *op. cit.*, p. 74. Substantially the same point was made by Whittlesey, *op. cit.*, pp. 559–60. [2] Shaw, *op. cit.*

[3] Sayers, *op. cit.*, p. 89. He uses for illustrative purposes the case in which, "with a normal cash reserve of 10 against deposits of 100 the banks hold excess reserves equal to 10," and compares the effects of variation of reserve ratios with those of open-market operations.

of situation that is usually associated with the choice between lower reserve requirements and expansionary open-market operations. Thus, little solace remains to be drawn from the sharply limited applicability of the argument in question: under the conditions which (apart from international transactions) give rise to consideration of the choice between higher reserve requirements and restrictive open-market operations, commercial banks are "loaned up" to begin with. Under such conditions, higher reserve requirements do, in fact, generate a shock effect upon commercial banks—and, hence, upon prices and yields of government securities—that is greater than the impact of restrictive open-market operations.

This difference in shock effect means that it is pointless to argue that more extensive use could be made of higher reserve requirements if increases in requirements were made in very small fractions. Obviously, if a given increase in reserve requirements is minute, its shock effect is also likely to be minute. But this is hardly comforting. The point is that restrictive open-market operations generate less of a shock effect that correspondingly higher reserve requirements. Thus, if what is called for in a given case is *highly* restrictive central bank action, the difference in the shock effects of the two instruments will be of much greater significance than if only *slightly* restrictive action is needed. In brief, substantial increases in reserve requirements will produce substantial differential shock effects; negligible increases in reserve requirements will produce negligible differential shock effects.

The differential shock effect of higher reserve requirements results from the fact that, in conditions wherein commercial banks are "loaned up" to begin with, higher reserve requirements are more deleterious to commercial-bank earnings than are restrictive open-market operations. Unawareness of this fact is at the bottom of the opposition to the Federal Reserve's views on the variation of reserve requirements.

Our conclusions also weaken the argument that changes in reserve requirements are preferable to open-market operations on the ground that the former will reduce time lags in the transmission of the effects of monetary policy to the commercial banking system. If results of the variation of reserve requirements were the same as results of open-market operations in all respects but speed of transmission, preference for the former weapon over the latter would be quite plausible. We have seen, however, that there is an asymmetry in results, an asymmetry which militates against the increase of reserve requirements as a counter-inflationary measure. For one major objective of a restrictive monetary policy is to curb the expansion of commercial-bank credit to private borrowers. This objective is thwarted by the variation of reserve requirements in comparison with the use of open-market operations. When, in addition, differences between the two weapons in administrative flexibility and in ease of reversibility are taken into account, little is left of the case for the use of changes in reserve requirements as a short-period alternative to open-market operations.

V

Would it be proper to conclude that adoption of the reserve-variation weapon is entirely without merit? [1] Not quite.

In economies where a government-securities market is lacking or where the central bank's portfolio of government securities is too meagre for the exclusive use of open-market operations, the additional weapon of changing reserve requirements is well nigh indispensable. But, as the government-securities market in such economies expands over time—owing, say, to a growing national debt in the course of governmental deficit financing of economic-development projects—open-market operations may become an increasingly realistic alternative to variation of reserve requirements.

Even in economies with an extensive government-securities market and with a central bank in possession of a relatively large securities portfolio, there may still be a distinct advantage in endowing the central bank with both weapons. Suppose that a country is experiencing, as was the United States in the mid 1930s, a heavy inflow of gold and that the monetary authority seeks to neutralise the potentially expansionary effect of this inflow upon the domestic money supply. To absorb the increase in commercial banks' excess reserves by means of open-market operations would imply a substantial reduction in the central bank's portfolio of government securities and, hence, a rise in the net interest burden of government debt. On the other hand, to absorb the increase in excess reserves by means of higher cash-reserve requirements for commercial banks would leave the central bank's security portfolio intact and would avoid any addition to the net interest burden of government debt. Thus, the variation of reserve requirements is not devoid of all merit as an instrument of monetary control, particularly in a world of open economies.

The Johns Hopkins University, Baltimore JOSEPH ASCHHEIM

[1] This is a conclusion reached by Professor Milton Friedman. With reference to the Federal Reserve System, he states, " The tools we now have are in large measure an accidental outgrowth of past history and are by no means best suited to present day needs. The rediscount tool is technically defective as an instrument of stabilization policy. Its effects are difficult to judge and it tends to be discontinuous in operation, so producing speculation in advance of changes. From the point of view of stabilization policy, it would seem to me desirable to eliminate completely the Federal Reserve's rediscounting powers. For similar reasons, control over reserve requirements is a defective instrument of stabilization policy. If we are to have legally prescribed reserves, they should, it seems to me, be fixed and not subject to change by administrative action.

" If rediscount powers and powers over reserve requirements were eliminated, monetary policy would be implemented predominantly through open-market operations. This is by all odds the best monetary instrument of stabilization policy at the present time. There is nothing that can be done by rediscount policy and changes in reserve requirements that cannot be made by open-market operations, and open-market operations are continuous in time and amount. They can be changed in amount from day to day and reversed if necessary frequently and without difficulty. And one ball is easier to juggle than three." Milton Friedman, " Consumer Credit Control as an Instrument of Stabilization Policy," in Board of Governors of the Federal Reserve System, *Consumer Instalment Credit*, Part II, Volume 2: *Conference on Regulation* (Washington: National Bureau of Economic Research, 1957), pp. 100–1.

SOME THEORY OF AN IGNORANT MONOPOLIST[1]

ALTHOUGH no one doubts the importance of limited information and uncertainty as factors affecting the management decisions of actual business firms, economists have long been reluctant to introduce such complications into traditional models of price and output determination. To the extent that learning and uncertainty phenomena have been considered at all, they have been studied mainly in the context of recent developments in the mathematical theory of games of strategy; and results reached via this route have had little if any effect upon the thinking of the majority of professional economists, most of whom continue, as before, to think and work in terms of models which presuppose a world of perfect information, perfect certainty and instantaneous response to changing circumstances.

The purpose of this paper is to initiate a redress of the balance: to encourage study of learning processes and delayed responses *within the framework of traditional economic theory*. The paper is directly concerned only with certain natural and potentially fruitful dynamical generalisations of traditional monopoly theory, but it has implications for every branch of established price theory. The discussion rests more on plausible than on demonstrative reasoning, and the mathematical argument is correspondingly elementary. However, some of the economic considerations which motivate the formal analysis are fairly subtle and are developed, accordingly, with more than usual care.

I. THE KNOWLEDGEABLE MONOPOLIST: STATICS

In keeping with the spirit, if not the letter, of traditional theory, consider a market in which, during any specified period of time, t, the quantity of *output* $x(t)$ of a particular commodity is produced by a monopoly firm and offered for sale to a group of prospective buyers at the unit *price* $p(t)$. Assume that *market demand* (monopoly *sales*) in the same period, denoted by $D(t)$, is a given function of price: $D(t) = D[p(t)]$; and suppose that *total costs* incurred by the monopolist in producing and selling the quantity $x(t)$ are given by a function $C(t) = C[x(t)]$. Assume also that the function D has a

[1] A complete list of acknowledgments would be embarrassingly long, but it would be indecent to ignore the obligations which I owe to my colleagues R. B. Heflebower, A. A. Charnes, Alvin L. Marty and Mitchell Harwitz, to Alan Walters of the University of Birmingham and to Arnold Zellner of the University of Washington. I am grateful also to the Graduate School of Northwestern University for partial but generous financial support. The research underlying this paper was undertaken for the project on *Temporal Planning and Management Decisions Under Risk and Uncertainty* at Northwestern University, under contract with the U.S. Office of Naval Research (contract Nonr—1228(10), Project 047-021). Reproduction of this paper in whole or in part is permitted for any purpose of the United States Government.

unique inverse, F, and provisionally define the *profit function* of the monopolist to be

$$R(t) \equiv F[x(t)]x(t) - C[x(t)] \qquad (0 \leqslant R(t))$$

(this function being presumed to be continuously differentiable). Finally, let the output of the monopolist in period t be set at any value of $x(t)$, in the domain of definition of the profit function, which satisfies the " maximum profit " condition

$$(1) \quad F[x(t)] + x(t)F'[x(t)] - C'[x(t)] = 0;$$

and let the price of the monopolist in period t be determined by the " market clearance " requirement

$$(2) \quad p(t) - F[x(t)] = 0.$$

Provided the profit function has a unique maximum value (this will henceforth be taken for granted), the conditions (1) and (2) then determine unique *equilibrium values* $\bar{x}$ and $\bar{p}$ of output and price which the monopolist will select and maintain in any and every market period. For purposes of reference, call the system (1), (2), *Model I.*

The preceding remarks provide a complete and accurate account of the essentials of traditional monopoly theory.[1] If the exposition has less " sparkle " than the usual classroom discussion of the same topic, that is probably because the typical instructor adds dynamical and other " frills " to his account of the statical theory. Among other things, it is usual to ask such questions as: " What happens if the monopolist makes a mistake and sets his output at some level other than $\bar{x}$? " Strictly speaking, however, it is not legitimate to ask this kind of question in the context of the model described by (1) and (2). For these equations (and the assumptions underlying them) assert unequivocally that *the monopolist has no choice:* either values of output and price $\bar{x}$ and $\bar{p}$ satisfying Model I exist and are set accordingly; or such values do not exist, in which case monopoly output and price are not specified by the model. To make the question meaningful, it is necessary to relax the assumptions of the theory, tacitly if not explicitly.[2] Let us relax them explicitly.

II. The Knowledgeable Monopolist: Dynamics

Let condition (2) in Model I be retained, but replace (1) by the alternative requirement

$$(3) \quad x(t) - x(t-1) = \lambda\{F[x(t-1)] + x(t-1)F'[x(t-1)] - C'[x(t-1)]\},$$

where λ is a positive constant; *i.e.*, suppose the output of the monopolist varies from one period to the next in direct proportion to the difference

[1] Compare J. R. Hicks, " Annual Survey of Economic Theory: Monopoly," *Econometrica*, Vol. III (1935), pp. 2–4.

[2] See, *e.g.*, the discussion of " stability " at pp. 3–4 of the Hicks article just cited.

between " marginal revenue " and " marginal cost." To determine output in any period t, it is only necessary to adjoin to (3) an initial condition specifying the value of output in the preceding period; equation (3) then generates a well-defined time series in $x(t)$ and the " market clearance " condition (2) may be used to obtain a corresponding time series in $p(t)$. Call the system described by (2) and (3) *Model II.*

It is now possible to pose the question stated earlier, for Model II does not *require* that output be set at some equilibrium value $x = \bar{x}$ as is the case in Model I. Of course, if output should just happen to be set at the value $\bar{x}$, the expression in braces in equation (3) will be zero and output will not change over time; *i.e.*, $\bar{x}$ is a stationary solution of (3) (which means that Model II is *statically equivalent* to Model I). If output is initially specified to have a value different from $\bar{x}$, however, output (and price) will vary in every subsequent period of time in accordance with (2) and (3). In the neighbourhood of an equilibrium point $(\bar{x}, \bar{p})$, for example, variations in output, starting from some initial value x_0 of x are approximately described by the equation

$$x(t) = \bar{x} + (x_0 - \bar{x})[1 + \lambda(F' + F'' - C'')]^t.$$

From this it is easy to verify that, except for special functions F and C, or special values of the " adjustment coefficient " λ, the approach to equilibrium is always gradual, may be oscillatory, and never terminates; also, that output will tend toward its equilibrium value over time for most " plausible " choices of the functions F and C, provided λ is not too large.

Model II is more interesting in several respects than Model I; in particular, since it is capable of generating states $[x(t), p(t)]$ other than the single equilibrium state $[\bar{x}, \bar{p}]$, it is potentially referable to empirical data sets of the kind which one associates with " going concerns " in the real world. Since it is still a rather dull affair, however, another " classroom " question is in order. Let it be the following: " What happens if the position of the market demand curve shifts? "

From one point of view, this question presents no new problem. Starting from a situation of monopoly equilibrium, and assuming that a shift in the demand function occurs, we have simply to deal with the question " What happens if the output of the monopolist is not set at the equilibrium level? " For the original value of output is simply one of many non-equilibrium values after the shift in demand occurs. In this respect, Model II is more satisfactory than might have been supposed at first sight.

From another point of view, however, the model gets into difficulties. So long as one deals with a fixed demand function, it is reasonably sensible to suppose that the profit and price calculations of the monopolist are made with reference to this function; one has merely to assume that the model applies to a situation in which, following various trial-and-error experiments with different prices, the monopolist knows the precise character of market

demand (at least within some relevant range of price and output quantities). If the demand function is not assumed to be fixed, however, Model II would seem to have definite *empirical* relevance only on a *recurrent* basis; *i.e.*, it would be devoid of practical interest, following any shift in demand, until it could be supposed that a " sufficiently long " interval of time had elapsed to permit the monopolist to discover the new position and properties of the demand function.

Now, a model of the latter type is rather awkward to justify; but if one is to retain Model II, the only obvious alternative to some kind of " recurrent validity " hypothesis is to suppose that the monopolist has *correct a priori knowledge* about market demand in every period of time. Since this postulate imputes a highly artificial type of omniscience to the monopolist, however, it can easily prove more embarrassing than the assumption it is designed to replace. On this view, it seems desirable to seek a generalisation of Model II. A " knowledgeable monopolist " is clearly just a theoretical curiosity; it is time to see whether an " ignorant monopolist " might not be something more.

III. The Ignorant Monopolist: Naïve Strategy

As a preliminary to the development of a more general model, return for a moment to the " recurrent validity " theme and assume that, *although the position of the demand function shifts from time to time, its general form remains always the same.* Specifically, assume that the function is always representable as a straight line of constant slope, say

$$(4) \quad D(t) = ap(t) + b,$$

where $a < 0$ and the constant b is a " shift parameter." If the demand function is assumed to shift not too frequently (no more often, say, than every three market periods), it is then permissible to argue that the monopolist will acquire *correct information* about the *slope* of the function from *ex post* price and sales data (the three-period scheme allows time for two consecutive observations so that a conjecture concerning slope can be made, and then time for a final observation to " confirm " the conjecture!). This having been accomplished, the monopolist's only remaining problem is to find the *position* of the demand function in different periods of time. In the latter connection, however, it is possible to argue that the monopolist—having settled on a definite slope—will *estimate* the current position of the demand function on the basis of recent sales experience—say by reference to sales and price in the immediately preceding market period. But this, added to our earlier assumptions, is formally equivalent to postulating the existence of a (linear) *conjectural demand function* of the form

$$(5) \quad E(t) = a\{p(t) - p(t-1)\} + D[p(t-1)],$$

where the symbol $E(t)$ denotes *conjectured sales* (in period t).[1] Then, since the coefficient a in (5) is assumed to have the same value as the corresponding coefficient in (4), it follows that the *conjectural demand function in period t* is identically the same as the *market demand function in period* $t-1$. Hence, if the revenue function in Models I and II is replaced by a " conjectural " revenue function involving the inverse of the conjectural demand function (5), monopoly output and price behaviour following a shift in market demand may still be analysed in terms of our earlier discussion. It is only necessary to argue as if shifts in the market demand parameter, b, occur one period later than is actually the case. In particular, the conditions for market equilibrium in Models I and II are entirely unaffected by the suggested substitution of a " conjectural " for an " objective " profit function.

Suppose, however, that the monopolist is less clever than the preceding discussion suggests, or that shifts in the market demand function occur so frequently that no amount of ingenuity permits the monopolist to obtain *correct* information about the form of the market demand relation. Specifically, suppose the monopolist believes the market demand function is linear when it is actually non-linear. Suppose, also, that the conjectural demand function of the monopolist is still given by (5). Then the position of the conjectural demand function in any given period is no longer determined simply by the position of the market demand function in the preceding period; its position depends also upon the behaviour of market price. To be sure, the conjectural demand function in period t will always coincide *at a single point* with the market demand function in period $t-1$; but unless the two functions coincide everywhere (which cannot happen on our present assumptions) a change in price (resulting, *e.g.*, from a change in output) leads to a shift in the conjectural demand function even though the position of the market demand relation is fixed.

It follows from this that the output and price behaviour of an " ignorant " monopolist (*i.e.*, a monopolist who manages to make only a " reasonable " guess about the form of the market demand function) cannot be described by working with the simple " lagged " version of Model II described above.

Let us carry out a modification of Model I to allow for the presence of a minimal degree of ignorance and see where it leads. First, replace the function F (which indicates the maximum unit price at which prospective buyers *will* purchase any given level of output) by an appropriate con-

[1] The notion of a " conjectural," or " imagined " demand, or " demand as seen by the seller," is, of course, a standard feature of traditional theories of oligopoly and pure competition; and it has been applied before to problems of monopoly. See Sidney Weintraub, " Monopoly Equilibrium and Anticipated Demand," *Journal of Political Economy*, 1942, pp. 427 ff.: N. Kaldor, " Market Imperfection and Excess Capacity," *Economica*, 1935; R. Triffin, *Monopolistic Competition and General Equilibrium Theory*, pp. 62–6; Bushaw and Clower, *Introduction to Mathematical Economics* Chapter Seven, Sections 4–5; S. Thore, F. Billstrom and O. Johansson, " Models Involving Monopolistic Strategies for Price and Supply," *Report 2 of the Uppsala Econometric Seminar* (mimeographed, no date); Cyert, Feigenbaum and March, " Models in a Behavioral Theory of the Firm," *Econometrica*, October 1958, pp. 611–12 (abstract).

jectural price function. In the present context, the " appropriate " concept is evidently a function which indicates the maximum unit price at which the monopolist *conjectures* that buyers will purchase any given level of output. Denoting the conjectural price variable by $e(t)$, therefore, and recalling that the conjectural demand function is defined by (5), the relevant conjectural price function is

$$(6) \quad e(t) = p(t-1) + k\{D(t-1) - x(t)\},$$

where $k \equiv -(1/a) > 0$. The " profit maximisation " requirement in Model I is then replaced by the condition

$$(7) \quad p(t-1) + k\{D[p(t-1) - 2x(t)\} - C'[x(t)] = 0,$$

while the " market clearance " requirement is replaced by its " conjectural " equivalent

$$(8) \quad p(t) - p(t-1) - k\{D[p(t-1)] - x(t)\} = 0.$$

The system (7), (8)—call it *Model III*—describes the behaviour of what might be called an " ignorant but opinionated " monopolist. The precise position and form of the demand function is conjectured " with certainty " at the beginning of any given period on the basis of sales and price data in the immediately preceding period. Just as in Model I, monopoly output and price are then assumed to be set in period t at certain unique " momentary equilibrium " values, $\tilde{x}(t)$ and $\tilde{p}(t)$ in order to yield a *conjectural* " profit maximum " and achieve *conjectural* " market clearance." And this procedure is assumed to apply in every market period regardless of the fact that, in general, *sales* conjectured by the monopolist, corresponding to any selected value of price, differ from *sales actually realised.*[1]

The working of Model III is illustrated in Fig. 1, where D represents the market demand function, C_m represents the marginal cost function, e is a line having the same *slope* as the conjectural price function and S describes the *conjectural supply function* of the monopolist,

$$\tilde{x}(t) = S\{\tilde{p}(t);\ (p(t-1) + kD[p(t-1)])\},$$

defined implicitly by equations (7) and (8). Sales are initially assumed to be set at the level D_0 corresponding to the price p_0; the conjectural price function at the *beginning* of period t_1 is therefore represented by e_1. The value $\tilde{x}$ of output which " momentarily " maximises conjectured profit in period t_1 is then $\tilde{x}_1$, as defined by the intersection of e_1 with the conjectural supply curve S (where " conjectural marginal revenue " is equal to " marginal cost "). The value $\tilde{p}$ of price at which it is conjectured that the

[1] This " portrait of a monopolist " is very similar to the " portrait of a monopolistic competitor " as drawn by Professor Chamberlin in his *Theory of Monopolistic Competition* (see, particularly, pp. 89–93). Indeed, it is possible to regard Model III as a basis for construction of a theory of multiple-market monopoly systems which are formally identical to systems considered by Professor Chamberlin, except that they do not depend on artificial " symmetry " conditions and can be considered to apply to " small-group " as well as " large-group " systems. But it is not within the scope of this paper to pursue the matter further.

market will be cleared in period t_1 is similarly defined as $\tilde{p}_1$. At the price $\tilde{p}_1$, however, sales in period t_1 are $\tilde{D}_1$ rather than D_0; so the conjectural price function at the beginning of period t_2 is represented by e_2 instead of e_1. Hence output in period t_2 increases to the level $\tilde{x}_2$, while price rises to $\tilde{p}_2$. And so the process continues, with consequences indicated by following the "zig-zag" line *ABCD* . . . As in the case of Model II, the adjustment

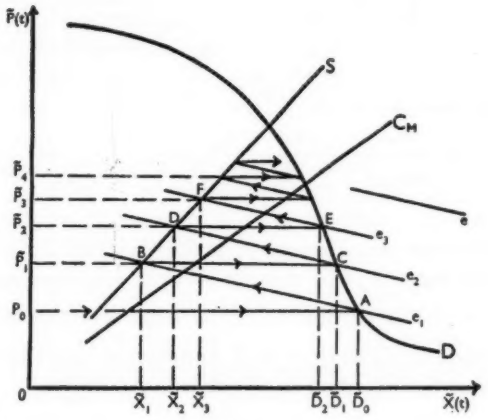

FIG. 1

process does not terminate, but the motion illustrated in Fig. 1 clearly tends toward the "true" equilibrium point $(\bar{x}, \bar{p})$—where the conjectured position [1] of the price function is consistent with realised results.

Analytically, the behaviour of "momentary equilibrium" output in the neighbourhood of an equilibrium point $(\bar{x}, \bar{p})$ is described by

$$(9) \quad x(t) = \bar{x} + (x_0 - \bar{x})\{(k/a + 1)(1 - k/(2k + c)\}^t \qquad (a < 0 < k;\ 1/a = F'[\bar{x}],\ c = C'[\bar{x}]),$$

which indicates that the motions of the system are "direct" and stable (as illustrated in Fig. 1) if $k \leqslant -a$, "oscillatory" and either stable or unstable if $k > -a$. Examples of the last two possibilities are illustrated in Fig. 2. The analogy with familiar "cobweb" models is amusing as well as instructive.

Model III has a number of intriguing features. In the first place, it permits monopoly equilibrium to be defined by the intersection of "curves of demand and supply." [2] Even more significant is the fact that the dynamical

[1] Notice that market equilibrium does not require the *slope* of the conjectured price function to bear some definite relation to the *slope* of the demand function; merely that the equilibrium values of output and price should satisfy both relations simultaneously.

[2] Compare Chamberlin's discussion of the case of traditional monopoly theory at pp. 12–16 of the *Theory of Monopolistic Competition*; also E. R. Hawkins, "Note on Chamberlin's Monopoly Supply Curve," and "Reply" by Chamberlin, *Quarterly Journal of Economics*, 1939, pp. 641 ff.

(and so the comparative statics) properties of the model can be analysed in "supply and demand" terms using strictly traditional techniques of analysis.[1] This is most easily seen by working with a model in which

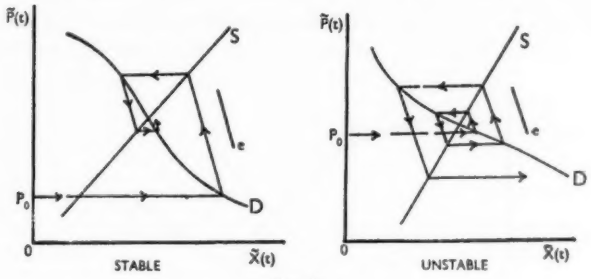

FIG. 2

units of measurement are chosen to make the equilibrium point $(\bar{x}, \bar{p})$ coincide with the point (0, 0). For, in this case, (9) implies that

$$\tilde{x}(t-1) \equiv \tilde{x}(t)/\{(k/a+1)(1-k/(2k+c)\} \equiv K\tilde{x}(t),$$

which means that the behaviour of "momentary equilibrium" price (as given by (8)) may be described in the alternative form

$$(10) \quad \Delta\tilde{p}(t) \equiv \tilde{p}(t) - \tilde{p}(t-1) = k\{D[\tilde{p}(t-1)] - K\tilde{x}(t-1)\}(k > 0).$$

And (10) asserts that price variations are directly proportional to *market excess demand*. The analogy with price behaviour in a competitive model requires no further emphasis.

IV. Quasi-Sophisticated Strategies

Although Model III is similar in many respects to a simple dynamical model of a competitive market, the analogy is not perfect. The clearest expression of a basic difference is probably provided by noting that Model III makes no provision for the possibility that the price conjectures of the monopolist may be *parametric*; in every case it is assumed that the coefficient k in the conjectural price function is strictly positive, which implies that the value of conjectured price, $e(t)$, depends on the value assigned to the output variable $x(t)$—that is to say, the price conjectures of the monopolist are *non-parametric*. Indeed, if these conjectures are assumed to be parametric in Model III (*i.e.*, if it is assumed that $k = 0$), (9) yields the value x_0 of $\tilde{x}(t)$ *for all values of* t, and (10) asserts that $\Delta\tilde{p}(t) \equiv 0$! This makes nonsense of the entire model.

It is instructive, however, to explore the consequences of this situation a little further. Assume, therefore, that $k = 0$ in Model III, that $\tilde{x}(t_0) = x_0$ and $\tilde{p}(t_0) = p_0$, but that $D[p_0] \neq x_0$ (x_0 and p_0 being given constants).

[1] For a description of the more usual situation in models of this kind, see Bushaw and Clower, *Introduction to Mathematical Economics*, pp. 186–7, 190–92.

Then, although the actual sales of the monopolist differ from desired sales (*i.e.*, output) in every period, the implication of Model III is that output and price will be maintained indefinitely at their initial levels, x_0 and p_0. Notice, however, that everything would be well *if the coefficient k in the conjectural price function were not also used as the "adjustment coefficient"* in (10). Specifically, if some positive constant, α, were substituted for k in (10), output variations would be governed by "marginal cost-price" considerations (the assumption $k = 0$ causes no trouble in (7)); and price variations would be governed by straightforward "market clearance" considerations. Moreover, this would seem to be a desirable solution on other grounds. There is no reason to assume that the monopolist is so naïve as to believe that his price conjectures in *any* given period will be correct; and in the absence of such an assumption, it is gratuitous to suppose that price is set *in every* period on the basis of the conjectural price function. It would be just as appropriate to describe price movements in terms of a strictly pragmatic "trial-and-error" process, as suggested above. Following the same line of argument, neither is there any reason to assume that output is set *in every period* to maximise conjectural profit as *currently* conceived; gradual adjustment "in the light of current conditions," as implied by one of the assumptions in Model II, would be an equally legitimate and in some ways a more satisfactory assumption.

In the light of the preceding remarks, it is in order to consider first the model

$$(7)\quad p(t-1) + k\{D[p(t-1)] - 2x(t)\} - C'[x(t)] = 0$$

$$(11)\quad p(t) - p(t-1) = \alpha\{D[p(t-1)] - x(t-1)\}\ (\alpha > 0),$$

which is simply Model III with a "price adjustment equation" in place of the "market clearance requirement" (8).

Equilibrium conditions for this model are precisely the same as for Model III, namely,

$$(7')\quad p + kD[p] - 2kx - C'[x] = 0,$$

and

$$(8')\quad D[p] - x = 0;$$

but the dynamical characteristics of the system are rather different.

This is conveniently illustrated in Fig. 3, where the curves D, S and e are defined precisely as in Fig. 1, and α is assigned the value $\alpha = 1/2$. Starting from values p_0, D_0 and x_0 of price, sales and output in period t_0, output in period t_1, denoted by $\tilde{x}_1$, is defined by the intersection of the curves e_1 and S. Price in period t_1 is determined, however, by the requirement (11); *i.e.*, $p(t_1) = p_0 + \frac{1}{2}(D_0 - x_0) = p_1$.[1] In period t_1, therefore, sales are D_1 and

[1] The "tilda" is dropped from the variable p in the present model because the values of $p(t)$ generated by (11) may not (and ordinarily will not) be "momentary equilibrium" values; *i.e.*, if a value $\tilde{x}(t)$ of x which satisfies (7) and a value of $p(t)$ which satisfies (11) are both inserted into (8') the resulting expression is not *identically* zero.

the conjectural price function at the beginning of the next period is e_2 (the " zig-zags " of Model III disappear because of the independence of price adjustment and price conjectures). Accordingly, output in period t_2 is $\tilde{x}_2$, price is given by $p_1 + (D_1 - \tilde{x}_1)/2 = p_2$ and so forth. In the present case,

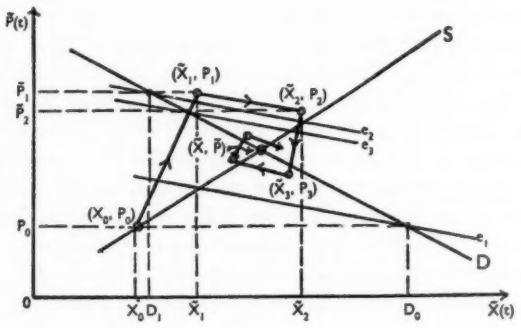

FIG. 3

the point $(\tilde{x}(t), \tilde{p}(t))$ " spirals " toward the equilibrium point $(\bar{x}, \bar{p})$; however, unstable " spirals " and stable and unstable " direct " motions are also possible.[1]

An even simpler graphical description of the model results if the now perfectly legitimate assumption is made that $k = 0$. The conjectural price function then becomes $e(t) = p(t-1)$, and (7) is replaced by the simpler requirement $p(t-1) - C'[x(t)] = 0$. Hence, output, price and sales adjustments can be described in terms that are formally identical with the usual text-book discussion of the adjustment of supply, demand and price in a purely competitive market.[2]

The system (7), (11) is a distinct improvement over Model III from the standpoint of potential empirical applications. Models which yield abrupt changes in the values of theoretical variables are hard to reconcile with

[1] The characteristic equation of the linearised counterpart of (7), (11) is

$$f(r) \equiv \begin{vmatrix} r - \alpha/a - 1 & \alpha \\ k/a + 1 & -r(2k + \epsilon) \end{vmatrix} = 0, \ (1/a = F'[x];\ \epsilon = C'[\bar{x}])$$

which is a *quadratic*. The corresponding equation for Model III, however, is

$$f(r) \equiv \begin{vmatrix} r - k/a - 1 & k \\ k/a + 1 & -(2k + \epsilon) \end{vmatrix} = 0,$$

which is *linear*. This " explains " the qualitative difference between the motions generated by the two models.

[2] What is even more surprising, J. N. Wolfe, of the University of Toronto, tells me that he has come across a case in Canada, involving monopoly suppliers of road-construction materials, which might be considered to fit this " competitive monopoly " model! Needless to say, I don't believe him.

known sets of economic data; hence it is desirable to avoid building models in which " cobweb " phenomena are, so to speak, part of the basic structure. Model (7), (11) is not altogether free of this defect, but because of the presence of the coefficient α in equation (11), it can always be made to yield a " smoother " time series than Model III for given functions C, D and a given value of k. Besides adding an element of flexibility to an otherwise rigid model, moreover, the presence of the coefficient α in (11) divorces the problem of estimating the coefficients of the conjectural price function from the problem of estimating the coefficients of the price-adjustment equation. Finally, the model describes " hit and miss " price behaviour of a kind which one might well find in a " going concern "; this means that fewer questions would have to be begged before attempting to use the model to interpret concrete situations.

Comments similar to the above apply with equal or greater force to the model described by

$$(11)\quad p(t) - p(t-1) = \alpha\{D[p(t-1)] - x(t-1)\} \qquad (\alpha > 0)$$

$$(12)\quad x(t) - x(t-1) = \beta\{p(t-1) + kD[p(t-1)] - 2kx(t-1) - C'[x(t-1)]\} \qquad (\beta > 0),$$

which is obtained from the preceding model by substituting an " output adjustment equation " for the " maximum profit " condition (7). The resulting system is still relatively simple from a formal point of view,[1] but it can be used to describe quite complicated patterns of price, output and sales behaviour. Because it portrays the monopolist as a *profit seeker* rather than a *profit maximiser*, moreover, it is an improvement on earlier models from a casual empirical point of view.

Indeed, if the model (11), (12) is generalised slightly to make allowance for the possibility that the monopolist varies output in direct response to changing demand conditions, it leaves little to be desired as an elementary description of the mechanics of monopoly output and price adjustment in a world of limited knowledge. This generalisation is accomplished very easily by adding an extra term to the right-hand side of (12), to obtain

$$(12')\quad x(t) - x(t-1) = \beta\{p(t-1) + kD[p(t-1)] - 2kx(t-1) - C'[x(t-1)]\} + \gamma\{D[p(t-1)] - x(t-1)]\}(\beta, \gamma > 0).$$

While the model described by (11) and (12′) is then equivalent, *from a statical point of view*, to all of the Models III, (7)–(11), and (11)–(12), its qualitative dynamical properties are in better accord with everyday experience. Formal analysis of these properties is left to the interested reader,

[1] The characteristic equation of the linearised version of (11), (12) is

$$f(r) \equiv \begin{vmatrix} r - \alpha/a - 1 & \alpha \\ -\beta(k/a + 1) & r + \beta(2k + c) + 1 \end{vmatrix} = 0,$$

where a and c are defined as before. Compare the corresponding equation for the system (7), (11) given in footnote 1, p. 714.

however, with the warning that the task is difficult and not particularly instructive.[1]

IV. Conclusion

Ignorance about demand conditions is a ubiquitous feature of market life in the real world. Whatever its source—whether it arises from competition among sellers, lagged response of buyers to changing prices, exogenous changes in tastes, techniques, government policies, etc.—it is a factor to be reckoned with in any attempt to describe observed output and price behaviour. Moreover, the phenomenon of demand ignorance implies—in a plausible if not in a demonstrative sense—the existence of learning and response mechanisms through which output and price decisions arrived at on the basis of provisional conjectures are revised in the light of realised results and ultimately reconciled with experience. The purpose of this paper has been to provide an intuitively plausible and logically coherent description of some particularly simple mechanisms of this kind.

In this connection, it is worth emphasising that the orientation of the preceding analysis is not entirely philosophical. On the contrary, the argument has been influenced at every stage by tacit recognition of the fact that the lack of provisionally plausible theoretical models of other than " purely competitive " markets has long been a major obstacle to systematic empirical study of the production and marketing activities of individual business firms. Whether any of the models outlined above can be used to obtain fruitful results in this area is, of course, another question. It may be noted, however, that most of these models have the econometrically convenient properties of linearity and identifiability. Also, most of them owe their existence to an earlier (and unsuccessful) attempt to reconcile traditional monopoly theory with factual descriptions of the behaviour of " monopolistic " firms in the United States.[2] Finally, preliminary analysis of price, sales, output, and inventory data obtained from a selected sample of major U.S. firms has yielded results which are sufficiently promising to encourage deeper and more systematic studies in the same direction. While the immediate emphasis of the present paper is theoretical, therefore, its original sources, current uses and ultimate aims are all distinctly practical.

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[1] The relevant characteristic equation (cf. footnote 1, p. 715) is

$$f(r) \equiv \begin{vmatrix} r - \alpha/a - 1 & \alpha \\ -\beta(k/a + 1) - \gamma/a & r + \beta(2k + c) + \gamma - 1 \end{vmatrix} = 0,$$

[2] A particularly instructive study of this kind is by Edmund P. Learned, " Pricing of Gasoline: A Case Study," *Harvard Business Review*, Vol. XXVI, November 1948, pp. 723–56.

MALTHUS AND KEYNES—A RECONSIDERATION[1]

I

In the past few years the view has solidified that Malthus was one of the most important precursors of the Keynesian way of thinking. This view was given its main impetus by Keynes himself, first in the memoir on Malthus[2] and later in the *General Theory*.[3] It bears testimony to the revolutionary nature of Keynes' contribution that prior to his work very little attention was paid to this element in Malthus' thought. This is true not only of general histories of economic thought,[4] but also of the specialised works devoted solely to Malthus. Bonar,[5] for example, devoted only a few pages to Malthus' treatment of the possibility of overproduction, whilst Palgrave gave only a paragraph to this topic.[6]

There are, of course, one or two exceptions to this early appraisal of Malthus' work; John Lalor is perhaps the most interesting of these, and as early as early as 1851 he was prepared to rate Malthus' theorising in this field higher than his contribution to population economics.[7] But in general a glance at the standard histories before the " Keynesian Revolution " will confirm that this aspect of Malthus was largely ignored.

It is often said that the question of overproduction was, after all, only a minor part of Malthus' total contribution and hence did not warrant much consideration, but there is very little evidence for this view. It is one of the predominant themes in the Ricardo–Malthus correspondence and occupies nearly a third of Malthus' *Principles*.

Keynes changed all this. Malthus' fear of over-saving now receives an honourable mention in the " new " histories, and some writers claim to have found many of the " building blocks " of the *General Theory* in Malthus. In particular, the writings of Hansen, Lambert and O'Leary may be mentioned.[8]

[1] I should like to acknowledge the advice of Professor Lionel Robbins, Mr. Piero Sraffa, Mr. Kurt Klappholz and Mr. G. C. Archibald in the preparation of this paper.

[2] " Robert Malthus: the First of the Cambridge Economists," *Essays in Biography* (1933).

[3] *The General Theory of Employment, Interest and Money*, Chapter 26.

[4] *E.g.*, L. H. Haney, *History of Economic Thought* (1917), p. 204, " As to overproduction, Malthus differed with the majority of his contemporaries in believing it possible as a general condition, his moral being that there are limits to parsimony or saving. He was clearly in error."

[5] James Bonar, *Malthus and his Work* (1924).

[6] *Palgrave's Dictionary of Political Economy* (Ed. by H. Higgs, 1925), Vol. 11, article on Malthus.

[7] John Lalor, *Money and Morals* (Preface). Mention should also be made of J. M. Robertson's *The Fallacy of Saving* (1892).

[8] A. H. Hansen, *Business Cycles and National Income*; Paul Lambert, " Keynes und Malthus," *Weltwirtschaftliches Archiv*, 1955; Preface to J. Stassart, *Malthus et la Population*; J. J. O'Leary, " Malthus and Keynes," *Journal of Political Economy*, 1952; " Malthus's General Theory of Employment and the Post-Napoleonic Depressions," *Journal of Economic History*, 1943. Professor Lambert seems to have taken up the most extreme position on this point. He argues that " le

In my view this attempt to set up Malthus as a forerunner of Keynes is of doubtful validity. My criticism is not merely that the functional relationships making up the theoretical framework of the *General Theory* are not to be found in Malthus. It is more fundamental than this. I also object to the view, stated so strongly by Keynes, that " the almost total obliteration of Malthus' line of approach and the complete domination of Ricardo's for a period of a hundred years has been a disaster to the progress of economics." [1]

In what follows I shall investigate rather more closely than has so far been the case the exact nature of Malthus' dispute with the orthodox school, and seek to show, first, that Malthus was not in fact an analytic forerunner of Keynes, and second, that victory for Malthus in his dispute with Ricardo over the effects of capital accumulation would probably not have hastened the development of Keynesian economics.[2]

II

Various parts of the Keynesian system have in their time been highlighted as the "essence" of the contribution. For example, the consumption function has been described by some writers as the key to Keynes' break with the Classics,[3] but clearly this is not so. With a mechanism to ensure the equality of planned saving and investment, the fact that a percentage of national income is not consumed does not necessarily present any problems of aggregate demand. On the other hand, there does seem to be general agreement about the fact that one of the important differences between Keynes and the Classics concerns the assumptions made about the savings-investment mechanism. Professor Paul Samuelson has put this point of view clearly in the following terms:

> " I myself believe the broad significance of the *General Theory* to be in fact that it provides a relatively realistic, complete system for analyzing the level of effective demand and its fluctuations. More narrowly, I conceive the heart of its contribution to be that subset of its equations which relate to the propensity to consume and to saving in relation to offsets-to-saving. In addition to linking saving explicitly to income, there is an equally important denial of the implicit ' classical ' axiom that motivated investment is *indefinitely expansible or contractable*, so that

parenté entre les deux auteurs est plus intime encore que Keynes en l'a cru." The reasons he gives for this view appear to ignore the question of the *ex ante* equality—or otherwise—of savings and investment. Cf. the Preface to Stassart, *op. cit.*, p. 14. Professor Robbins allowed me to see some correspondence he had had on this subject with Professor Lambert which was helpful in clarifying this issue.

[1] J. M. Keynes, *Essays in Biography*, pp. 140–1.

[2] There are, of course, certain contemporary economists who have never accepted this new verdict on Malthus; R. F. Harrod, for example, has written, " I cannot believe that Malthus, splendid as he was as a population theorist, contributed much of value to economics, in which he was always muddled." *The Life of John Maynard Keynes*, p. 460.

[3] " It has been my conviction for years that the great contribution of Keynes' *General Theory* was the clear and specific formulation of the consumption function." A. H. Hansen in *The New Economics* (ed. S. E. Harris), p. 135.

whatever people *try* to save will always be fully invested. It is not important whether we deny this by reason of expectations, interest rate rigidity, investment inelasticity with respect to over-all price changes and the interest rate, capital or investment satiation, secular factors of a technological and political nature, or what have you. But it is vital for business-cycle analysis that we do assume definite amounts of investment which are highly variable over time in response to a myriad of exogenous and endogenous factors, *and which are not automatically equilibriated to full employment saving levels by any internal efficacious economic process.*" [1]

Now an essential feature of Classical macro-economics was the equality of saving and investment; the Classics were prepared to admit the possibility of a breakdown in the economic system if uncompensated hoarding were to be attempted, but generally speaking, its occurrence on any significant scale was thought to involve an assumption of irrational behaviour.[2] At the root of Classical (and Neo-Classical) economics was the "saving is spending" theorem, and this clearly has to be sharply contrasted with the Keynesian approach. As Keynes himself pointed out:

> "Contemporary thought is still deeply steeped in the notion that if people do not spend their money in one way they will spend it in another. Post-war economists seldom, indeed, succeed in maintaining this standpoint *consistently*; for their thought today is too much permeated with the contrary tendency and with facts of experience too obviously inconsistent with their former view. But they have not drawn sufficiently far-reaching consequences; and have not revised their fundamental theory." [3]

It is against this background that we must look at Malthus' contribution.

III

The first point to make clear is that throughout his analysis Malthus assumed that an act of saving was always translated into an act of investment, that is to say, he assumed the equality of *ex ante* saving and *ex ante* investment. Thus in this respect Malthus was at one with the orthodox Classical school; as Professor Robbins has remarked, "unfortunately Malthus had insisted that savings were always invested; he spurned the suggestion that they were often hoarded." [4]

Take the following quotation from Malthus:

> "A third very serious error of [Say, James Mill and Ricardo] and practically the most important of the three, consists in supposing that

[1] Paul A. Samuelson, "The General Theory (3)" in the *New Economics* (ed. S. E. Harris), pp. 150–1.

[2] This statement requires qualification: there are instances in the classical literature of discussions of the effects of sudden changes in the demand for money. Perhaps the *locus classicus* here is J. S. Mill's, "Of the Influence of Consumption upon Production."

[3] J. M. Keynes, *The General Theory of Employment, Interest and Money*, p. 20.

[4] L. C. Robbins, *Robert Torrens and the Evolution of Classical Economics*, p. 248. See also Schumpeter, *History of Economic Analysis*, p. 641 n.

accumulation ensures demand; or that the consumption of the labourers employed by those whose object is to save, will create such an effectual demand for commodities as to encourage a continued increase of produce.

"Mr. Ricardo observes, that 'If £10,000 were given to a man having £100,000 per annum, he would not lock it up in a chest, but would either increase his expenses by £10,000, employ it himself productively, or lend it to some other person for that purpose; in either case demand would be increased, although it would be for different objects. If he increased his expenses, his effectual demand might probably be for buildings, furniture, or some such enjoyment. If he employed his £10,000 productively, his effectual demand would be for food, clothing, and raw materials, which might set new labourers to work. But still it would be *demand*.'

"Upon this principle it is supposed that if the richer portion of society were to forego their accustomed conveniences and luxuries with a view to accumulation, the only effect would be a direction of nearly the whole capital of the country to the production of necessaries, which would lead to a great increase of cultivation and population. But this is precisely the case in which Mr. Ricardo distinctly allows that there might be a universal glut; for there would undoubtedly be more necessaries produced than would be sufficient for the existing demand."[1]

It is clear from this passage that Malthus assumed that savings "became" investment, and to reiterate within such a model it is not possible to deduce that lack of effective demand is caused by oversaving.[2] What problem, then, was it that really troubled Malthus? His starting-point was the (obvious) fact that unemployment was a normal feature of capitalist economies. "The actual wealth of all states with which we are acquainted is very far short of their powers of production."[3] Somehow or other this situation was due to "overproduction" caused by a deficiency of effective demand.

At this point it is important to distinguish two possible interpretations of the concept of overproduction. First, "general" overproduction caused by an excess of planned saving over planned investment—which was ruled out by assumption both in Malthus and orthodox Classical reasoning. Secondly, the idea that the translation of "revenue into capital" led to a falling rate of profit and if continued would kill the motive for further accumulation. It is in this second sense that Malthus' fears must be interpreted; assuming profits to be determined by a simple relationship between investment and consumption, then a switch from consumption to investment would (inevitably) produce a lower monetary return than had been expected: if profits had only been "normal" to begin with, the transfer would result in a stagnation in economic activity. The way out of such a situation, Malthus argued, was to stimulate "unproductive" consumption. To

[1] *Principles of Political Economy*, 2nd ed., pp. 322–3.

[2] For our purposes it is immaterial whether we assume that savers are entrepreneurs, or that there is a flexible rate of interest. Classical macro-economics contains both assumptions.

[3] Malthus, *op. cit.*, p. 329.

summarise this view in Malthus' own words: "it appears to be quite . . . certain that an inordinate passion for accumulation must inevitably lead to a supply of commodities beyond what the structure and habits of . . . society will permit to be profitably consumed." [1]

Using this second definition of overproduction, it is easy to show that Malthus and Ricardo were in basic agreement concerning its logical possibility, a point which comes out clearly in their correspondence. The typical case Malthus considered was the switch to a higher rate of capital accumulation financed by means of a decrease in luxury spending; continued for a sufficient length there would be a point where nobody wanted the extra output, the rate of profit would be below the minimum necessary for net capital accumulation.

In a sense Ricardo was in complete agreement with all of this, provided that this hypothetical case of Malthus' really did occur. "I affirm with you," he wrote to Malthus, "that 'if the farmer has no adequate market for his produce, he will soon cease to distribute more necessaries to his labourers,' with a view to the production of more necessaries." [2]

And when Malthus wrote to say:

> "I also fancy that I am fortified with new arguments to prove demonstratively that a neat revenue is *absolutely impossible* under the determination to employ the whole produce in the production of necessaries, and consequently that if there is not an adequate taste for luxuries and conveniences, or unproductive labour, there must necessarily be a general glut." [3]

Ricardo replied: ". . . with a very slight alteration I should entirely concur in your proposition." [4]

Moreover, Ricardo agreed that if Malthus' diagnosis were correct, interventionist measures to maintain total output were required.[5]

So far I have argued that in so far as the dispute between Malthus and Ricardo was about the possibility of a rate of profit insufficient to promote capital accumulation, the two writers were in basic agreement, but a great deal of the heat generated in the controversy was due to the fact that they did not realise this. Malthus fought for a confession that conditions could be thought up which would produce a general glut; Ricardo, equally fervently, regarded such conditions as unrealistic. As usual, a great deal of the misunderstanding was due to imprecise language and terminology, and in addition one cannot help feeling that neither side had a particular desire to end the controversy—it was, after all, one way of keeping the friendship going.

[1] Malthus, *op. cit.*, p. 325.
[2] *Works of David Ricardo* (ed. P. Sraffa), Vol. VIII, p. 278. Letter dated October 9, 1820.
[3] *Ibid.*, Vol. VIII, p. 285. Letter dated October 26, 1820.
[4] *Ibid.*, Vol. VIII, p. 300. Letter dated November 24, 1820.
[5] For more on Malthus' attitude towards public works see the present author's "The Theory of the Economic Effects of Public Expenditure in English Classical Economics," *Economica*, 1958.

Each accused the other of wilful refusal to understand; Ricardo complained to James Mill:

> " I think he [Malthus] has not understood himself, for what are all his attacks on Say and me, surely not because we have said in all cases there would be motives sufficient to push production to its utmost extent . . . " [1]

Whilst Malthus's apparent frustration appears in a letter to Ricardo:

> " I am either unfortunate in my explanations, or your mind is so entirely prepossessed with your own views on the subject of our discussion, that you will not give to any statement, which departs from them the degree of attention which is necessary to put you in possession of what is meant." [2]

What has been said so far may be summarised in the following manner: Malthus' main concern was with the effects of a declining rate of profit caused by " excessive " capital accumulation. Unfortunately within the framework of orthodox Classical theory a falling rate of profit was not necessarily a disequilibriating force. The rate of interest (a real phenomenon determined mainly by the rate of profit) equated saving and investment, hence a change in it merely altered the allocation of total spending between consumption and investment. As Keynes himself pointed out, Malthus had no alternative theory of interest to offer, so that ultimately his idea of excessive capital accumulation rested on the assumption that investment was independent of its rate of return.

IV

In this section some of the policy implications of Malthus' analysis will be examined, to elaborate the point that he had much less in common with Keynes than is usually supposed.

First of all, what did Malthus have to say about the effect of wage cuts on the volume of employment? It will be recalled that within the framework of the *General Theory* direct effects are zero—although there is some possibility of secondary effects via the rate of interest. Malthus, on the other hand, thought that full employment would eventually be achieved through wage reductions. In writing of the post-1815 situation he mentions the consequences of a " sudden abundance of corn," it reduced prices,

> " the consequence of which was that, in the midst of plenty, thousands were thrown out of employment—a most painful but almost unavoidable preliminary to a fall in the money wages of labour, *which it is obvious* could alone enable the general income of the country to employ the same number of labourers as before, and, after a period of severe check to the increase of wealth, to recommence a progressive movement." [3]

[1] *Economica*, 1958, Vol. IX, p. 13. Letter dated July 9, 1821.
[2] *Ibid.*, Vol. IX, p. 90. Letter dated October 9, 1821.
[3] Malthus, *op. cit.*, p. 393, my italics.

This is straightforward Classical reasoning with the idea of a " wages fund " lurking in the background.

Let us now take a look at the " Malthusian policy for full employment," for it is here that the apparent Keynesian nature emerges most strikingly; probably it is the use of the term " effectual demand " that has misled people. Malthus argued (in clear contrast to Keynes) that, starting from a low level of economic activity, the only way to increase effective demand was to raise consumption or " unproductive expenditures." As he put it: " the greatest stimulus to the continued production of commodities, taken altogether, is an increase in the exchangeable value of the whole mass, before a greater value of capital has been employed upon them." [1]

Or in more explicit terms: " a country with great powers of production should possess a body of consumers who are not themselves engaged in production." [2]

All this followed from Malthus' concern with the effects of a falling rate of profit, and the possibility of an equilibrium situation [3] where further investment was unprofitable.

> " It is undoubtedly possible by parsimony [Malthus wrote] to devote at once a much larger share than usual of the produce of any country to the maintenance of productive labour; and suppose this to be done, it is quite true that the labourers so employed are consumers as well as those engaged in personal services, and that as far as the labourers are concerned, there would be no diminution of consumption or demand. But . . . the consumption and demand occasioned by the workmen employed in productive labour can never *alone* furnish a motive to the accumulation and employment of capital."

The only expenditure that investment created was the wages of the labour directly involved, hence—

> " it is impossible that the increased quantity of commodities, obtained by the increased number of productive labourers, should find purchasers, without such a fall of price as would probably sink their value below that of the outlay, or, at least, so reduce profits as very greatly to diminish both the power and the will to save." [4]

There seem to be some peculiar assumptions involved here: there are no multiplier effects and investment is replaced in total every period.

There is, finally, the question of the social control of the saving-investment process. Perhaps the most important implication of Keynes' work was the notion that in an unregulated capitalist economy, full employment was fortuitous; hence given maximum production as an end, government control of aggregate saving and investment was necessary. But Malthus did

[1] Malthus, *op. cit.*, p. 361.
[2] *Ibid.*, p. 398.
[3] A situation where only normal profits were being earned.
[4] *Ibid.*, pp. 314–15.

not think along these lines; for him *laissez-faire* was the guiding rule. At the end of the *Principles* he sums up the position thus:

> " Saving, as I have before said, is, in numerous instances, a most sacred private duty. How far a just sense of this duty, together with the desire of bettering our condition so strongly implanted in the human breast, may sometimes, and in some states of society, occasion a greater tendency to parsimony that is consistent with the most effective encouragement to the growth of public wealth, it is difficult to say; but whether this tendency, if let alone, be ever too great or not, no one could think of interfering with it, even in its caprices. There is no reason, however, for giving an additional sanction to it, by calling it a public duty. The market for national capital will be supplied, like other markets, without the aid of patriotism. And in leaving the whole question of saving to the uninfluenced operation of individual interest and individual feelings, we shall best conform to that great principle of political economy laid down by Adam Smith, which teaches us a general maxim, liable to very few exceptions, that the wealth of nations is best secured by allowing every person, as long as he adheres to the rules of justice, to pursue his own interest in his own way." [1]

V

The conclusion must surely be that victory for Malthus would not necessarily have led to the happy results that Keynes supposed. There appears to be little in the Malthusian analysis which bears affinity to the model of the *General Theory*. Needless to say, this does not imply that victory for the Ricardian approach was a " good thing." The fundamental weakness of Classical economics was its failure to work out a coherent analysis of the determination of income and employment in the short run. And whilst it is true that Ricardo, at least, emphasised that his analysis was meant as a contribution towards the theory of long-period equilibrium, one cannot escape the conclusion that by and large the orthodox Classical group were prepared to let this long-run analysis serve without modification as a description of the economic system at all points of time.

Malthus was undoubtedly a better observer of the economic scene than his opponents—he was aware that full employment was not the norm—but it cannot be said that his analysis provides an explanation of this phenomenon. Probably critics have been misled by Malthus' policy proposals, for to a certain extent they are not dissimilar to what a " Keynesian " would recommend. But a vague feeling that something is wrong does not justify the overthrow of a particular analytic framework. When a particular theory has been falsified it must be replaced with another theory, but Malthus did not do this, he did not break through the inner logic of Classical macro-economics.[2]

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[1] Malthus, *op. cit.*, p. 434.

[2] It may be added here that in the present writer's opinion this breakthrough was Keynes' greatest achievement.

A FURTHER NOTE ON FACTOR–COMMODITY PRICE RELATIONSHIPS

In a recent issue of this Journal (June 1958) Mr. R. F. Harrod has raised again the question of the effect of trade upon factor prices. As it is not intended to disagree fundamentally with Mr. Harrod, it would seem proper to give at once reasons for wearying the reader with further argument. These are:

(i) That there does remain an important difference of opinion between Mr. Harrod, on the one hand, and Professor Samuelson and the present writer, on the other, regarding the exact nature of the conditions required to ensure factor-price equalisation when the number of goods and factors exceeds two. I do not believe that these conditions can be expressed in terms of factor intensities as implied by two-dimensional accounts and as explicitly stated by Mr. Harrod.[1]

(ii) That the devastatingly restrictive nature of the conditions referred to, for many goods many factors, has been widely overlooked because of the general difficulty of the multi-dimensional argument.

(iii) That the only full proof of the general case so far published [2] can be very much simplified. In this connection there is also a small point of difference between the present writer and Professor Samuelson which it has seemed desirable to clear up in an appendix.

(iv) That it seems appropriate in this context to comment briefly on two points of methodology.

It is convenient to begin with the methodological complaints. First, following well-tried and undeniably useful habits of thought we tend, in economic argument, to assume that, even if our hypotheses are not precisely in accord with the facts, our results are still applicable *in some degree*. We suppose that, in general, our assumptions are reasonable approximations or simplifications which give insight into the problem. If they are not strictly met, then there may be impediments of one kind or another to the working of the mechanism, but the mechanism itself remains fundamentally unaltered.

Without wishing to question the value of this procedure in a general way it cannot be too strongly emphasised that it will, in some cases, let us down severely. Professor Samuelson's celebrated factor-price equalisation theorem [3] provides us with a case in point. Most of the assumptions upon which this theorem rests can be regarded as reasonable first approximations. This is not true, however, of the factor intensity assumption, to which Mr.

[1] Economic Journal, June 1958, p. 249.

[2] James and Pearce, "The Factor Price Equalisation Myth," *Review of Economic Studies*, 1951/52, p. 111.

[3] Economic Journal, June 1948, June 1949.

Harrod has recently directed our attention (*loc. cit.*). If this restriction on the production function does not apply in the real world there exists much more than a mere impediment to the complete realisation of factor-price equalisation. On the contrary, there exists a mechanism in its own right which could lead to precisely the *opposite* effect. Factor prices could move apart as a result of trade. A step-by-step account of how this might occur is given in the present writer's paper already cited. Moreover, the same considerations apply *a fortiori* if the more complicated conditions appropriate to the general case are not met. As far as the factor-price equalisation theorem is concerned therefore *everything* depends on precise correspondence between the assumptions and the facts of the real world.

My second methodological point concerns the assumption of two goods two factors only, in terms of which a great deal of economic theory is commonly set out. It is tempting to regard two-dimensional analysis as reasonable on the ground that the number of goods and factors is merely a matter of classification. If some measure of classification is inevitable, why not go all the way and fit all goods into one or other of two groups? The difficulties involved are no more than a facet of the index-number problem, which is always with us in any case.

The objection to this attractive argument is that there are times when the economic theory under review is itself no more than a facet of the index-number problem. The price of a composite commodity is precisely the weighted sum of the prices of the factors which produce it, the weights being the ratio of inputs to output. The factor-price equalisation theorem seeks to establish that the connection between factor prices and their weights is such that there is one, and only one, way of building up any given commodity price, at the same time fulfilling all the economic conditions for equilibrium. If the world in fact contains many factors, whose relative prices vary independently, then we are simply assuming away the major part of our problem by trying to fit it into two dimensions. We must not say that by a composite commodity price we mean a " suitable " price index, when the very question we are investigating is what weights are admissible and what are not? However complex and clumsy our methods may be, we have no alternative but to face the complications of the general case. To do otherwise is merely to engage in self-deception.

Accepting this necessity, we have reluctantly to conclude that the very clarity of Mr. Harrod's explanation of the factor-intensity hypothesis is, in a sense, unfortunate. This is because factor intensities are entirely irrelevant to the problem as soon as we pass to three or more goods and factors. The whole concept of factor intensity in fact turns out to be, in this context, a particularly disastrous red herring; for the supposition that factor intensities do not reverse themselves seems to have been widely regarded as empirically likely and, accordingly, the factor-price equalisation theorem has come to be accepted, even by economists who would readily concede the existence in

the real world of more than two factors of production as defined above. Thus it may be that support for the theorem rests, either upon the belief that arbitrary classification of goods and factors cannot affect the issue, or upon an incomplete understanding of the n dimensional complications. In short, Mr. Harrod's case, strong though it is, is weaker than it need be. It seems important to make it clear that the likelihood of factor-price equalisation is immeasurably reduced by the addition of more goods and factors, not simply because there are now more factor intensities which might reverse themselves, but because the whole nature of the necessary condition on the production function is transformed and becomes progressively more restrictive with each extra dimension. An attempt is made to explain this below.

As the extent of the controversy suggests that what follows may have interest even for non-specialists, it is assumed throughout (except in the appendix) that the reader is not familiar with the languages of topology and matrix algebra. Apologies are tendered to those who are. For the same reason I have tried to develop analogies which I hope will gain in clarity what they lose in rigor.

For the sake of brevity we use the following notation. x_{ij} is the quantity of the ith factor used in the production of the jth good. ϕ_j is the output of the jth good and q_j is its selling price. Now Professor Samuelson's assumption of constant returns to scale means that the total value of every product will be just exhausted by the total paid out to the factors which produce it whatever the output. In our notation therefore we may write down n equations as under, where n is the number of goods and factors.

$$\sum_{i=1}^{n} p_i x_{ij} = q_j \phi_j \quad (j = 1, 2, \ldots n)$$

[$p_i x_{ij}$ is, of course, the bill for the x_{ij} units of the i^{th} factor used in the production of product j. $\sum_{i=1}^{n} p_i x_{ij}$ means the sum of all the bills for all factors from 1 to n used to produce j. This is equal to the value of output $q_j \phi_j$ of the jth good. There are n of these equations, one for each j.]

Now we know that the quantities x_{ij} of factors used are determined by factor prices p_i and commodity prices q_j so that our n equations can be thought of as n equations in the $2n$ variables p_i, q_j. That is to say, given the q_j, we may solve for all p_i or, given p_i, we may solve for all q_j. Professor Samuelson's proof that trade will equalise factor prices is essentially a proof that these n equations have one, and only one, solution. That is, there is one and only one set of p_i corresponding to each set of q_j, and vice versa. The necessity for factor-price equalisation follows at once from this; for the assumption of identical production functions in all countries means that the same set of n equations will do for all countries. And, since trade equalises commodity prices as between countries and there is one, and only one, set of corresponding factor prices, factor prices also must be equalised.

Normally, of course, a set of equations will have more than one solution unless they are of a particular form. For example, $q = p^2$ has two solutions: $q = +p$ or $q = -p$. Any given value of q has two corresponding values of p, not one. If our n equations above are of a form which gives rise to multiple solutions countries may have different factor prices with the same commodity prices so that factor-price equalisation is not a necessary consequence of trade. Our problem in short is to find the condition which ensures that our equations have only one solution.

Consider for a moment the simple example $q = p^2$. For most values of q the two corresponding values of p will be very different; *e.g.*, when $q = 9$, p will be either $+3$ or -3. The difference between the two p's is 6. But we can always choose some value of q such that the two solutions are very close; *e.g.*, when q equals one over 1 million the corresponding p's will be $+0{\cdot}001$ and $-0{\cdot}001$. The difference between solutions is reduced to $0{\cdot}002$. We can in fact say that we can find some values of p and q in the region of which a small change in p (namely from $+0{\cdot}001$ to $-0{\cdot}001$) *leaves q unchanged* whilst still satisfying the equation $q = p^2$.

This principle is of very general validity and applies to all types of equations likely to have any economic significance.[1] If a set of n equations in n variables p_i and n arbitrarily chosen constants q_j have more than one solution (not close together) for any one set of q_j it will usually be possible to choose some other set of constants q_j such that the solutions are close together. Conversely, if there exists, for any q_j, solutions close together, it will usually be found that at least two solutions exist for any set of q_j for which a real solution is possible.

To put this another way, where the form of the equations we have to solve is such that we can find *some* set of factor and commodity prices such that a small change in factor prices leaves commodity prices unchanged, then our equations will in general give rise to multiple solutions for *any* set of commodity prices. If, on the other hand, we can be sure that *every* small change in factor prices must lead to some change in commodity prices, then there will be one, and only one, solution for each set of commodity prices.

We now apply this principle to our equations.

$$\sum_{i=1}^{n} p_i x_{ij} \equiv q_j \phi_i \quad (j = 1, 2, \ldots n)$$

As we are interested only in values of p, x, q and ϕ, which satisfy these equations, any small change in the left-hand side must be equal to the corresponding change in the right-hand side. Thus

$$\sum_{i=1}^{n} (dp_i x_{ij} + p_i dx_{ij}) = dq_j \phi_j + q_j d\phi_j \quad (j = 1, \ldots n)$$

where a d preceding any variable is read " the small change in."

[1] It is not, however, universally true. Readers who have doubts are invited to turn to the appendix to this note.

[It will be recognised that the principle followed in writing the change in $p_i x_{ij}$ as $dp_i x_{ij} + p_i dx_{ij}$ is precisely the same as that invoked when we show the marginal revenue of a monopoly to be made up of the change in sales multiplied by price plus the (negative) change in price multiplied by sales.] But we know already that:

$$\sum_{i=1}^{n} p_i dx_{ij} = q_j d\phi_j \quad (j = 1, 2, \ldots n)$$

for factor prices p_i are equal to the marginal-value products of the factors. These last equations merely state that the value of the change in the product is equal to the sum of the marginal-value products of the new factor quantities employed.

Hence by subtraction

$$\sum_{i=1}^{n} dp_i x_{ij} = dq_j \phi_j \quad (j = 1, \ldots n)$$

Now we have argued above that whether factor prices are equalised or not depends on whether or not we can find some p_i and q_j (*i.e.*, some x_{ij}) such that a set of small changes dp_i leave q_j unchanged. That is, if we can find some x_{ij} such that in the above equation all dq_j are zero so that

$$\sum_{i=1}^{n} dp_i x_{ij} = 0 \quad (j = 1, \ldots n) \quad . \quad . \quad . \quad . \quad . \quad \text{(i)}$$

then multiple solutions are possible and factor prices need not be equalised. If, on the other hand, no such set of x_{ij} is possible, then only one solution of our equations exists and factor-price equalisation is inevitable. Mathematicians will recognise at once that a non-zero solution to equations (i) will exist if, and only if,

$$|x_{ij}| = 0$$

Hence the condition for factor-price equalisation given in my paper (*loc. cit.*).

For those unfamiliar with the relevant mathematical theorem equations (i) are set out in full for the two-factor case. Multiple solutions demand that for one set of values of p_i and q_j the x_{ij} should be such that

$$x_{11}dp_1 + x_{21}dp_2 = 0$$

and

$$x_{12}dp_1 + x_{22}dp_2 = 0$$

Given the x's, what we now have is two equations in two variables dp_1 and dp_2. These equations are also of a particular kind. In geometric terms they are straight lines with slopes $\frac{x_{11}}{x_{21}}$ and $\frac{x_{12}}{x_{22}}$ which cut only at the origin where all dp's are zero. In general, all dp's zero, is the only simultaneous solution of these equations, a fact which merely tells us that if we do not change factor prices we will not change commodity prices either whatever the value of the x's.

Our two equations can have a solution with non-zero dp's, however, if

the two straight lines are coincident. That is, if they have the same slope so that

$$\frac{x_{11}}{x_{21}} = \frac{x_{12}}{x_{22}}$$

This is, of course, the factor-intensity condition, where the ratios of the inputs of factors one and two are the same for both products. This means that, in the region where factor intensities are equal, any small change in factor prices will leave commodity prices unchanged. If such a region exists factor-price equalisation is not inevitable.

This geometrical interpretation can be extended to the case of three factors three goods. The three equations (i) now become three *planes* whose common point is the origin (zero dp's) as before. (Three planes will, of course, in general cut only in a single point.) If we think of the point of intersection (the origin) as the corner of a room, then the slopes of the lines formed by the intersection of each plane with walls and floor will be numerically equal to factor intensities. If all three planes were coincident factor intensities would be the same in all cases. But the important point is that for a non-zero solution we do *not* require all three planes coincident. This is much too strong a condition. Three planes may intersect in more than one point without being coincident. Two planes intersect in a straight line. If the third plane cuts the second in the same straight line as the first, then all three planes have a *line* in common. It may in fact be possible to choose non-zero dp's which satisfy the three equations (i) even if the three planes are not coincident, that is, even if there is no point where factor intensities are the same in all cases. Moreover, this may be possible even if no factor intensities are ever at any time the same for any two goods.

To find the true condition which ensures a non-zero solution to equations (i) we imagine that our x's are now variables and the dp's are constants. Since the dp's are the same in all three equations, then all three must be the same equation and the x's must be values satisfying it. The graph of this single equation must again be a plane through the origin, the slopes of which are determined this time by the dp's. If the x's all satisfy this equation they must lie on this plane.

We now see geometrically how the factor-price equalisation condition generalises. In two dimensions (measuring x's along the axes) the point x_{11}, x_{21} must never lie on the same straight line through the origin as the point x_{12}, x_{22}. In three dimensions the points x_{11}, x_{21}, x_{31}; x_{12}, x_{22}, x_{32}; and x_{13}, x_{23}, x_{33}; must never be on the same *plane* through the origin. This condition cannot be explained in terms of factor intensities. Written in full, it requires that the expression,

$$x_{11}(x_{22}x_{33} - x_{23}x_{32}) - x_{12}(x_{21}x_{33} - x_{23}x_{31}) + x_{13}(x_{21}x_{32} - x_{22}x_{31})$$

should never be zero for any values of p and q. This is a condition highly unlikely to be met even if the factor-intensity condition is. To show this consider again the two-dimensional case.

Let us represent the production functions for two commodities in terms of single equi-product contours showing combinations of inputs which will produce one unit of each good. On a single large playing-field we set out two race-tracks in the shape of these iso-product curves and set a spectator at a point representing the origin. Two runners set off, one on each track, their speed of running being determined by the curvature of the tracks. If at any time during the race the runners and the spectator lie on a straight line, *i.e.*, if the two runners appear as one to the spectator, this is equivalent to the case where trade does not necessarily tend to equalise factor prices.

The three-dimensional analogue would be three aircraft flying at different speeds along curved tracks at different and varying heights. The three aircraft must never lie in the same plane as a fixed control tower on the ground. That is they must never *appear* to the observer in the control tower to lie in a straight line.

It is evident that there are vastly more degrees of freedom in the three-dimensional case than the two. In the first place, since we now have equi-product surfaces rather than curves, our aircraft must be thought of as taking part in an infinity of races, one for each possible track across the equi-product surfaces. If at *any* time in *any single* race the aircraft appear to the observer to be in a straight line this is equivalent to the breakdown of the factor-price equalisation theorem. Moreover, there are more degrees of freedom in any single race in the three-dimensional case than in the two. To demonstrate this think of two runners on two straight parallel tracks as two-dimensional and three runners on three parallel tracks as three-dimensional. The runners do not begin from the same start, but they run at different speeds. In the two-dimensional case if one runner overtakes and passes the other this is the analogue of the breakdown of factor-price equalisation. In three dimensions the breakdown occurs not as we might think if the runners come three abreast or if any two runners pass one another. It occurs if the three ever at any time lie in a straight line. This can happen whether any one runner passes any other or not.

Finally, attention is drawn to the fact that as we increase the number of goods and factors above three we increase still further the number of degrees of freedom. It is not only that we cannot express the factor-price equalisation condition in terms of factor intensities when there are more than two goods. We must take note also of the fact that the condition on production functions becomes more and more restrictive as we increase the dimensions. To assume that it is met is merely to assume what we want to prove.

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Appendix

The reader who has followed Professor Samuelson's brief account of the general case [1] treated above may be disturbed by what appears to be a difference of opinion. It has been contended here that

$$|x_{ij}| \neq 0$$

is a necessary and sufficient condition for factor-price equalisation, whereas Professor Samuelson seems to argue that it is not even sufficient. Conceding that this condition is both necessary and sufficient to exclude the possibility of multiple solutions of our equations in the region of a point, he expresses a doubt whether it is so in the large. He goes on to prove that one *sufficient* condition for uniqueness in the large includes the additional requirement that there should be some numbering of factors such that the leading principal minors of $|x_{ij}|$ of every order should be non-zero also.

The key to the apparent contradiction lies in my contention that, if a set of equations for one value of the parameters has more than one solution in the large, it will usually be possible to choose some value of the parameters such that the new equations have multiple solutions in the region of a point. If this were universally true, conditions necessary for the exclusion of multiple solutions in the region of a point must exclude also multiple solutions in the large, provided they hold for all possible values of the variables.

In fact, Professor Samuelson's extra conditions cover cases where solutions exist in the large which cannot be brought together by any change of parameters. For example, the equation

$$\tan p - k = 0 \quad (k \text{ constant})$$

has an infinity of distinct solutions p', p'', etc., such that $p' - p'' = 2\pi$ whatever the value of k. But this arises solely out of the fact that the curve tan p has an infinite number of parts. Tan p becomes infinite for finite values of p.

The interested reader may care to consider the properties of the following pairs of equations

$$\left.\begin{aligned}(p_1 - a)(p_2 - a) - q_1 &= 0 \quad q_1 \neq 0\\ (p_1 - a)^2 - (p_2 - a)^2 - q_2 &= 0 \quad q_2 \neq 0\end{aligned}\right\} \quad \ldots \quad \text{(i)}$$

$$\left.\begin{aligned}\tan p_1 - \tfrac{1}{2}p_2 - q_1 &= 0\\ \tfrac{1}{2}p_1 - p_2 - q_2 &= 0\end{aligned}\right\} \quad \ldots \quad \text{(ii)}$$

Equations (i) have two solutions for every set of q. Equations (ii) have an infinity of solutions for every set of q. In case (i) the Jacobian (see Professor Samuelson, *loc. cit.*) does not vanish (equivalent to $|x_{ij}| \neq 0$), but all principal minors do. In case (ii) neither the Jacobian or its minors can ever vanish whatever the values of p, but one minor does become infinite. It would seem from case (ii) that even Professor Samuelson's sufficient conditions are not really sufficient if any minor of the Jacobian can become infinite.

The common feature of these examples is that at least one equation can be satisfied with one p infinite, leaving the remaining prices finite. Now it would seem economically unsound to suppose that our functions should be such as to allow a finite set of commodity prices to generate some infinite factor prices. Thus we can exclude cases where the functions have two or more parts on purely economic grounds.

In short, I hold that the assumption that only infinite commodity prices can give rise to infinite factor prices is a suitable alternative to Professor Samuelson's conditions on the minors of the Jacobian. If we make this assumption the additional condition $|x_{ij}| \neq 0$ is necessary and sufficient to ensure factor-price equalisation both in the small and in the large as I have argued. Unfortunately, as we have seen, even this modified condition is highly restrictive and unlikely to be met.

[1] *Review of Economic Studies*, Vol. XXI, 1953/54, p. 9, and Appendix 3, p. 16.

PROTECTION, REAL WAGES AND REAL INCOMES[1]

1. In a recent article in this JOURNAL[2] on "Protection and Real Wages: A Restatement," Mr. Lancaster has re-examined the famous Stolper–Samuelson theorem and concluded:

> "This paper does not deny that protection will raise the real wage of one of the factors, but shows that no general statement about which of the factors this will be can be deduced from the relative 'scarcity' of the factors in the Stolper–Samuelson sense.
>
> "Although the Stolper–Samuelson theorem 'Protection raises the real wage of the scarce factor' is shown to be an incorrect generalisation, a restatement in the form 'Protection raises the real wage of the factor in which the imported good is relatively more intensive' has general validity."

It is proposed in Section I of this paper to review systematically the original Stolper–Samuelson contribution, therewith to advance a critique (distinct from Mr. Lancaster's criticism, which is not accepted), of the Stolper–Samuelson formulation of the theorem and then to restate the theorem: this restatement being considered to be the only true and general statement about the effect of protection (prohibitive or otherwise) on real wages of factors in the context of the basic Stolper–Samuelson model. The logical truth of the restated theorem is briefly analysed then in the context of alternative models. Section II proceeds to extend the scope of the discussion with the argument that, with a non-prohibitive tariff, a sharp distinction must be drawn between the impact on the real wage of a factor and the effect on its real income; some implications of this distinction are then analysed.

I. PROTECTION AND REAL WAGES

2. In the following analysis, we shall take the *basic* Stolper–Samuelson model to mean that the protecting country has two factors, two commodities enjoying different factor intensities, linear and homogeneous production functions subject to diminishing returns (along isoquants) and incomplete specialisation in production. Full employment of factors, pure competition and perfect mobility of factors are also assumed.

Founded on this model, we have three alternative formulations of the theorem concerning the impact of protection on the real wages of factors:

(1) *Restrictive Stolper–Samuelson Theorem.* "International trade necessarily lowers the real wage of the scarce factor expressed in terms of

[1] This paper was read to the Nuffield Economics Society. My thanks are due to Professor Hicks and J. Black for substantial help with the exposition of the paper. I am also happy to record my heavy indebtedness to Professor Harry Johnson, who has been generous with suggestions that have led to numerous improvements in the paper.

[2] ECONOMIC JOURNAL, June 1957, pp. 199–210. The following quotation is from p. 199.

any good." [1] This formulation restricts itself to the comparison of the free-trade real wage with the self-sufficiency real wage of the scarce factor. The comparison is confined to the case of a prohibitive tariff and excludes non-prohibitive protection. The theorem can be rewritten as follows: prohibitive protection necessarily raises the real wage of the scarce factor.

(2) *General Stolper–Samuelson Theorem.* Protection raises the real wage of the scarce factor.[2] This formulation is clearly intended to be more general and includes non-prohibitive tariffs as well. To emphasise this, we may rewrite it thus: protection (prohibitive or otherwise) necessarily raises the real wage of the scarce factor.

(3) *Stolper–Samuelson–Metzler–Lancaster Theorem.* " Protection [prohibitive or otherwise] raises the real wage of the factor in which the imported good is relatively more intensive." [3]

In the ensuing analysis any reference to " the Stolper–Samuelson theorems " should be taken to relate to the initial two formulations alone; reference to the last formulation will always be by its full title.

3. We can begin by setting out the basic elements in the argument leading to the twin formulations of the Stolper–Samuelson theorem:

(1) protection increases the internal relative price of the importable good:

(2) an increase in the relative price of a good increases the real wage of the factor used intensively in its production;

(3) the importable good is intensive in the use of the scarce factor.

Therefore,

(4) protection raises the real wage of the scarce factor.

These arguments must each be closely examined.

4. Concerning argument (1), we must distinguish between prohibitive and non-prohibitive protection:

(i) Protection will necessarily raise the relative price of the im-

[1] Stolper and Samuelson, " Protection and Real Wages," *Readings in the Theory of International Trade* (A.E.A., Blakiston Co., 1949), p. 346.

[2] The actual formulation of the general Stolper–Samuelson theorem is from Lancaster, *op. cit.*, p. 199. While the bulk of their analysis relates explicitly to the restrictive formulation, there are several indications that Stolper and Samuelson had in mind the general formulation as well: (1) a large number of quotations they cite from other authors to outline the problem refer to tariffs in general rather than to tariffs of a prohibitive nature alone; (2) they feel it necessary to assume that " the country in question is relatively small and has no influence on the terms of trade. Thus any gain to the country through monopolistic or monopsonistic behaviour is excluded " (*op. cit.*, p. 344); this assumption is quite superfluous, as we shall later see, if we wish to sustain only the restrictive formulation of the theorem; and (3) the title chosen for the article is not " International Trade and Real Wages " but " Protection and Real Wages." Lancaster, *op. cit.*, p. 201, also construes the Stolper–Samuelson theorem in its general form; thus witness his argument that " Protection will cause a movement in the *general direction* $Q'Q$, away from the free-trade point towards the self-sufficiency point " (my italics).

[3] Lancaster, *op. cit.*, p. 199. This theorem has been given its stated name on grounds which are made explicit later.

portable good when the tariff is prohibitive; the free-trade relative price of the importable good is lower than under self-sufficiency.[1]

(ii) Non-prohibitive protection may either raise, leave unchanged or lower the internal relative price of the importable good. Metzler has demonstrated that this last " perverse " possibility will occur, in the context of our present model, when the elasticity of foreign demand for imports (η_x) is less than the domestic marginal propensity to consume exportable goods (c).[2] It follows, then, that if imports are not inferior goods in the protecting country's consumption this case requires inelastic foreign demand; and we can ensure that the internal relative price of the importable good always rises with the imposition of a tariff by assuming *either* elastic foreign demand (sometimes done in the form of assuming a small country) *or* a big enough tariff (in the limit, a prohibitive tariff) for demand to be elastic.

5. Argument (2) follows necessarily from the basic Stolper–Samuelson model. To show this simply, we should recall the technological features of the model employed by Samuelson some years later in this JOURNAL[3] to demonstrate factor-price equalisation: these features are identical with those of the Stolper–Samuelson model in all respects. We propose thus to avoid altogether the use of the box-diagram and work instead with the unique relationships that Samuelson derived in these later articles between commodity price-ratios, factor price-ratios and factor proportions in the two industries in a country, from the given assumptions concerning technology alone. These are summarised in Fig. 1, which is reproduced, with slight changes, from Samuelson's 1949 article.[4]

Let L_C and L_F represent the labour employed in producing clothing and food respectively; T_C and T_F being the quantities of land so employed.

[1] This is true except in a *limiting* case where the terms of trade will not change with trade. This case, however, can be ruled out, in the context of the model used here, by assuming that the community indifference curves (used here without any welfare connotation) are strictly convex. This limiting case will henceforward be ignored.

[2] Metzler, " Tariffs, the Terms of Trade and the Distribution of National Income," *Journal of Political Economy*, February 1949, pp. 1–29. It should be emphasised that the Metzler formula for determining the impact of protection on the internal commodity price-ratio relates to the case where the initial situation is that of free-trade. Where, however, the initial situation itself has a tariff and the impact of *increased* protection is the subject of analysis, the " perverse " possibility mentioned in the text will occur, as argued in Section II, when a slightly altered condition is fulfilled. The discussion in Section I, however, is confined to initial situations of free trade, as with Stolper and Samuelson, Metzler and Lancaster.

[3] " International Trade and the Equalisation of Factor Prices," ECONOMIC JOURNAL, June 1948, pp. 163–84; and " International Factor-Price Equalisation Once Again," ECONOMIC JOURNAL, June 1949, pp. 181–97.

[4] This diagram is to be found on p. 188 of Samuelson's 1949 ECONOMIC JOURNAL article, *op. cit.* Full exploration of this diagram is to be found in an excellent article by Professor Johnson, " Factor Endowments, International Trade and Factor Prices," *Manchester School*, September 1957, pp. 270–83. Professor Johnson, however, works with a slightly adapted diagram, to be found in Harrod, " Factor-price Relations Under Free Trade," ECONOMIC JOURNAL, June 1958, pp. 245–55. On grounds of economy, the discussion of this well-known diagram has been kept brief in this paper.

W/R represents the ratio of wages to rents; L/T the factor endowment ratio of the country; and P_F/P_C the price of food over the price of clothing. Clothing is the labour-intensive industry, food the land-intensive industry, at all relevant factor price-ratios. ($L_C/T_C > L_F/T_F$ at all relevant W/R.)[1] As wages fall relatively to rents, the price of food is shown to rise relatively to that of clothing in a monotonic fashion. The factor endowment ratio of the country (L/T) fixes the range of the diagram which is relevant. This is a purely technology-determined diagram, and demand conditions are totally absent from it.

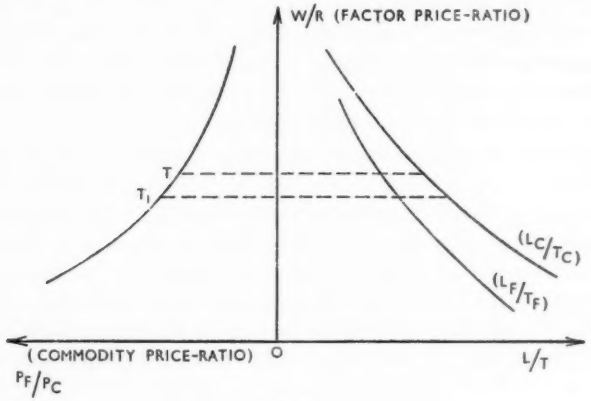


FIG. 1

T being any given commodity price-ratio (P_F/F_C), change it to T_1 such that the relative price of food rises. With it, the labour-to-land ratios in both food and clothing will rise. The marginal physical product of land in both products will thus rise and of labour fall, so that the real wage of land will be unambiguously increased and of labour decreased. Increase in the relative price of food thus increases the real wage of land, the factor intensively employed in its production; and reduces the real wage of labour, the factor intensively used in producing clothing (whose relative price has fallen).

This argument, it should be noted, rests on the assumption, part of the basic Stolper–Samuelson model, that the rise of the relative price of food does not go so far as to make the country specialise completely on food, in so far as the fall in the real wage of labour is concerned; for, once the country is specialised completely, further increases in the relative price of food will raise the real wage of *both* labour and land, which is destructive of the full validity of argument (2).

Given the basic Stolper–Samuelson model, therefore, an increase (de-

[1] Although the factor-intensities of the commodities may be reversible, they *cannot* reverse for a country with a *given* factor endowment. At the present stage of our argument, therefore, we do not need to make the strong assumption that factor-intensities are non-reversible at *all* factor price-ratios.

crease) in the relative price of a good will necessarily increase (decrease) the real wage of the factor intensively used in its production.

6. Argument (3) that the importable good is intensive in the use of the scarce factor is really the well-known Heckscher–Ohlin theorem. The crucial question that it raises is: does the Heckscher–Ohlin theorem follow from the basic Stolper–Samuelson model? To answer this question, we should first have to define "factor scarcity." We may choose from three alternative definitions of factor scarcity:

A. Lancaster Definition. A country's scarce factor is that which is used more intensively in the production of the importable good. This definition may be described as tautological, since it turns the Heckscher–Ohlin theorem into a valid proposition by *definition*. It may also be described as an internal definition, since it excludes any comparison with the foreign country. It has been suggested by Lancaster.[1]

B. Heckscher–Ohlin Definition. A country's scarce factor is that whose relative price is higher than abroad under self-sufficiency. This may also be described as a price definition, since the country's scarce factor is that factor which is more expensive prior to trade than abroad. This definition has been used by Heckscher and Ohlin.[2]

C. Leontief Definition. A country's scarce factor is that of which there are fewer physical units per unit of the other factor than abroad. This may also be described as a physical definition, since it defines scarcity with reference to the relative physical quantities of factors.[3]

Using each of these definitions in turn, let us analyse the Heckscher–Ohlin theorem.

A: If the Lancaster definition of factor scarcity is used, then the Heckscher–Ohlin theorem holds by definition.

B: If the Heckscher–Ohlin definition of factor scarcity is used then the further assumptions of international identity of production functions and non-reversibility of factor-intensities of commodites between the

[1] Lancaster, *op. cit.*, p. 208, argues that "the only acceptable definition" of a scarce factor is that which defines it as the factor "which is used more intensively in the good of which more is produced in isolation than in trade." It is of some interest to note that tariffs designed to influence distribution are probably set with reference to such internal criteria: to raise the real wage of labour, for instance, tariffs are imposed on labour-intensive industries rather than on products of industries using a factor which is scarcer at home than abroad; with the possible exception of the pauper-labour argument for such tariffs.

[2] For a convincing attribution of the authorship of this definition of factor scarcity to Heckscher and Ohlin, see a masterly article by R. Jones, "Factor Proportions and the Heckscher–Ohlin Theorem," *Review of Economic Studies*, 1956–57, pp. 1–10. The definition may be also illustrated in terms of Fig. 1: country A is labour-abundant and country B land-abundant if, under self-sufficiency, $(W/R)_A < (W/R)_B$.

[3] W. Leontief, "Domestic Production and Foreign Trade: The American Capital Position Re-examined," *Proceedings of the American Philosophical Society*, September 28, 1953; reprinted in *Economia Internazionale*, February 1954. Again, country A is labour-abundant and country B land-abundant if, under self-sufficiency, $(L/T)_A > (L/T)_B$.

two countries will suffice to ensure the full validity of the Heckscher–Ohlin theorem.[1]

C: If the Leontief definition of factor scarcity is used, then the threefold assumptions of non-reversibilities of factor-intensities of commodities between the trading countries and the international identity of both production functions and tastes will ensure the validity of the Heckscher–Ohlin theorem.[2]

7. We can now sum up on the Stolper–Samuelson formulations as follows: [3]

A. (1) The restrictive Stolper–Samuelson theorem is logically true if we use: (*a*) the basic Stolper–Samuelson model, and (*b*) the Lancaster definition of factor scarcity.

(2) The general Stolper–Samuelson theorem is logically true if we use the further assumption that the elasticity of foreign demand is greater than the marginal propensity to consume exportable goods ($n_x > c$).

B. (1) The restrictive Stolper–Samuelson theorem is logically true if we use: (*a*) the basic Stolper–Samuelson model, (*b*) the Heckscher–Ohlin definition of factor scarcity, (*c*) the assumption of international identity of production functions, and (*d*) the assumption of non-reversibility of factor-intensities of commodities between the countries.

(2) The general Stolper–Samuelson theorem is logically true if we use the further assumption that $n_x > c$.

C. (1) The restrictive Stolper–Samuelson theorem is logically true if we use: (*a*) the basic Stolper–Samuelson model, (*b*) the Leontief definition of factor scarcity, (*c*) the assumption of international identity of production functions, (*d*) the assumption of non-reversibility of factor-

[1] This can be seen readily from Fig. 1. If $(W/R)_A < (W/R)_B$ and production functions with non-reversible factor-intensities are common between the countries, then we can see that $(P_F/P_C)_A > (P_F/P_C)_B$ under self-sufficiency and the labour-abundant country *A* will necessarily export the labour-intensive commodity, clothing. We could, of course, specify what appears to be a less restrictive condition than that set out in the text: for instance, we could sustain the Heckscher–Ohlin theorem by assuming merely that, instead of identical production functions between countries, the differences in the production functions are not large enough to outweigh the effect of differences in factor scarcity on the pre-trade commodity price-ratios. We have preferred to use the strong condition (identity of tastes) instead of the weak one on the ground that the use of the latter seems to be bad methodology, amounting to the argument that the Heckscher–Ohlin definition of factor scarcity will suffice to sustain the Heckscher–Ohlin theorem if other factors do not work to invalidate it.

[2] The Heckscher–Ohlin theorem would not hold as a logically true proposition in this case unless we also postulate now international identity of tastes (or the weak postulate that differences in tastes between countries do not affect the issue). This follows from the fact that while, with identical production functions, country *A* will show a bias towards the production of the labour-intensive commodity, clothing, by virtue of her physical abundance in labour, this bias in production may be more than offset by a bias in *A* towards the *consumption* of clothing: such that, in self-sufficiency, we find that $(P_F/P_C)_A < (P_F/P_C)_B$ and country *A*, although physically abundant in labour, would export the land-intensive commodity, food.

[3] The phrase "logically true" in the following statements is used in the strict mathematical sense: "A statement that is true in every logically possible case is said to be *logically true*" (Kemeny, Snell and Thompson, *Introduction to Finite Mathematics* (Prentice-Hall, 1957), p. 19.

intensities of commodities between countries, and (e) the assumption of international identity of tastes.

(2) The general Stolper–Samuelson theorem is logically true if we use the further assumption that $n_x > c$.

A tree-diagram, based on this analysis, is presented in Table I.

8. We are now in a position to decide whether Stolper and Samuelson derived their theorems logically. Aside from their basic model:

(1) they adopt, though without complete clarity, the Heckscher–Ohlin definition of factor scarcity and the postulate concerning the non-reversibility of factor-intensities; and, quite explicitly, the assumption of international identity of production functions: [1] this establishes the restictive Stolper–Samuelson theorem as logically true (B(1));

(2) they further assume that " the country in question is relatively small and has no influence on the terms of trade "; [2] this establishes the general Stolper–Samuelson theorem as logically true (B(2)).

9. No critique of the Stolper–Samuelson formulations can thus be founded on the argument that they are not logically true, given the premises. What we could say, however, is that the theorem should be founded as closely as possible on the *basic* Stolper–Samuelson model alone; and

(1) that, if we use the Heckscher–Ohlin definition of factor scarcity, the assumptions that we find ourselves making about the international identity of production functions and the non-reversibility of factor-intensities to sustain the twin formulations of the Stolper–Samuelson theorem are, on this criterion, *restrictive*; and

(2) that, if we use the Leontief definition of factor scarcity (as we should probably want to since it is, in a sense, the most " objective " definition we could adopt in this context), we discover ourselves adopting the threefold restrictive assumptions (C(1)) of international identity of production functions and tastes plus the non-reversibility of factor-intensities of commodities, to sustain the Stolper–Samuelson formulations.[3]

10. It will be remembered, however, that these restrictive assumptions were made only because we wished to use argument (3) concerning the validity of the Heckscher–Ohlin theorem.[4] This may also be seen indirectly from

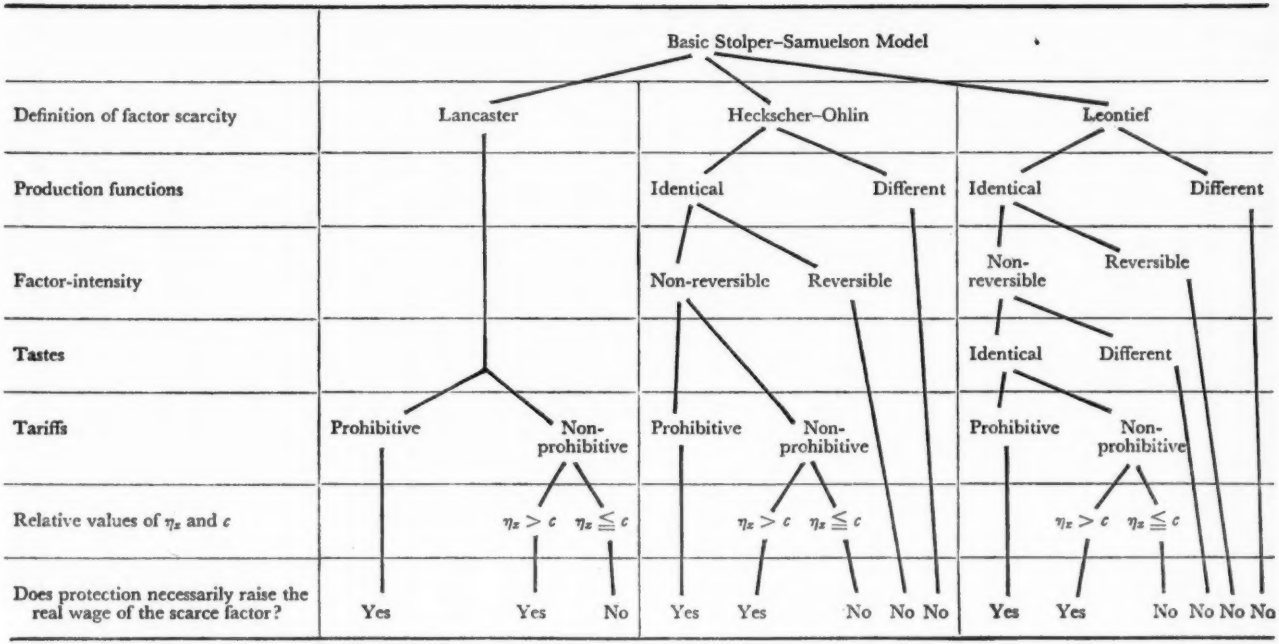
[1] Stolper and Samuelson, *op. cit.*, pp. 335–40. Some of the argument is, of course, obscure in view of the pioneering nature of the article: a sympathetic interpretation, therefore, is called for. Metzler, *op. cit.*, p. 5, also adopts the Heckscher–Ohlin definition of factor scarcity in discussing the Stolper–Samuelson theorem.

[2] Stolper and Samuelson, *op. cit.*, p. 346.

[3] The additional restrictive assumption that $\eta_x > c$ has not been listed here because we wish at this stage to concentrate on only those restrictive assumptions which are made to sustain argument (3).

[4] It is important to remember that these assumptions are restrictive only in so far as we wish to found our theorem exclusively on the basic Stolper–Samuelson model.

TABLE I


Basic Stolper–Samuelson Model
Definition of factor scarcity
Lancaster
Heckscher–Ohlin
Leontief
Production functions
Identical
Different
Identical
Different
Factor-intensity
Non-reversible
Reversible
Non-reversible
Reversible
Tastes
Identical
Different
Tariffs
Prohibitive
Non-prohibitive
Prohibitive
Non-prohibitive
Prohibitive
Non-prohibitive
Relative values of η_x and c
$\eta_x > c$
$\eta_x \leqq c$
$\eta_x > c$
$\eta_x \leqq c$
$\eta_x > c$
$\eta_x \leqq c$
Does protection necessarily raise the real wage of the scarce factor?
Yes
Yes
No
Yes
Yes
No
No
No
Yes
Yes
No
No
No
No

the fact that, if we use the Lancaster definition of factor scarcity, no such restrictive assumptions are necessary (A(1)): for the Heckscher–Ohlin theorem has been rendered valid by definition!

The suggestion follows readily from these considerations that we should reformulate our theorem in terms of arguments (1) and (2) alone, while eliminating the use of the troublesome argument (3). This can be done readily: protection (prohibitive or otherwise) raises the real wage of the factor intensively employed in the production of the importable good. This theorem is logically true if we use: (*a*) the basic Stolper–Samuelson model, and (*b*) the assumption that $n_z > c$.

This theorem has been described as the Stolper–Samuelson–Metzler–Lancaster theorem on the following grounds:

> (1) It is *implicit* in the Stolper–Samuelson argument, towards the end of their paper: " It does not follow that our results stand and fall with the Heckscher–Ohlin theorem. Our analysis neglected the other country completely. If factors of production are not comparable between countries, or if production functions differ, nevertheless, so long as the country has only two factors, international trade would necessarily affect the real wage of a factor in the same direction as its relative remuneration." [1]
>
> (2) Metzler *explicitly* states it as " the Stolper–Samuelson conclusion that tariffs benefit the factors of production which are required in relatively large amounts in the industries competing with imports." [2]
>
> (3) Lancaster advances this formulation directly as an *alternative* to the Stolper–Samuelson formulations considered above on the ground that it is more general than the latter.

11. Whereas, however, Lancaster's observation that the Stolper–Samuelson formulations are " non-universal " (restrictive) is well taken, the argument by which he supports it is erroneous and different from that set out in this paper. Lancaster proceeds by establishing, with the aid of a highly ingenious model, the proposition that, in the context of the basic Stolper–Samuelson model combined with the assumption of a small country facing fixed terms of trade, differences in demand conditions (" which good is the wage-good ") will affect the composition of a country's foreign trade. On this proposition he founds the following critique:

> " The non-universality of the [Stolper–Samuelson] theorem is due to incorrect formulation: if the scarce factor is defined as that which is used more intensively in the good of which more is produced in isolation

[1] Stolper and Samuelson, *op. cit.*, pp. 355–6. Homogeneity of factors between countries has not been listed separately as an assumption in this paper because it is believed that this is implicit in both the Heckscher–Ohlin and the Leontief definitions of factor scarcity.

[2] Metzler, *op. cit.*, p. 13. Metzler, of course, does not state it as a rival formulation, but it is abundantly clear that he is aware that this formulation is implicit in the general Stolper–Samuelson theorem.

> than in trade (the only acceptable definition), then the previous analysis has shown that different wage-goods may make for different factor scarcities. In this sense, the Stolper–Samuelson formulation is meaningless, since the phrases ' real wages . . . in terms of any good ' and ' scarce factor ' represent incompatible concepts." [1]

The following comments on Lancaster's critique seem warranted here, in view of our preceding analysis.

To begin with, it is difficult to understand what Lancaster means by the statement that " the previous analysis has shown that different wage-goods may make for different factor scarcities. In this sense, the Stolper–Samuelson formulation is meaningless, since the phrases ' real wages . . . in terms of any good ' and ' scarce factor ' represent incompatible concepts." Which good will be imported into a country will depend in our model on the pre-trade commodity price-ratios in the trading countries; these price-ratios are determined by domestic supply and demand; and domestic demand is affected by " which good is the wage-good." If the scarce factor is defined tautologously as that which is used intensively in the importable good it follows then, from elementary considerations, that " different wage-goods may make for different factor scarcities." But surely, how can this render the Stolper–Samuelson formulations *meaningless* or make ' real wages . . . in terms of any good ' and ' scarce factor ' *incompatible* concepts? And, more pertinently, why should this make the Stolper–Samuelson formulation " non-universal "?

Indeed, if the tautologous definition of factor scarcity is adopted, as Lancaster suggests, then the general Stolper–Samuelson theorem and the Stolper–Samuelson–Metzler–Lancaster theorem are *identical*: the phrases " scarce factor " and " factor intensively employed in the importable good " can be used interchangeably. Lancaster cannot, therefore, claim one formulation to be " non-universal " and the other to be " universally true ": on his own definition of factor scarcity, the two formulations come to the same thing!

To be sure, Lancaster's critique would be valid (though, as we have shown, incomplete) only if the physical, Leontief definition of factor scarcity were proven to have been adopted by Stolper and Samuelson, and were adopted by Lancaster as well; as formulated, however, the criticism is merely erroneous.[2] In failing to investigate precisely what Stolper and Samuelson assumed by way of their definition of factor scarcity, Lancaster has further by-passed the only legitimate critique that can be sustained against the actual formulation of the theorem by Stolper and Samuelson: namely, that advanced in this paper.

[1] Lancaster, *op. cit.*, p. 208.

[2] Lancaster has pointed out to me, in private communication, that he really had in mind the physical definition of factor scarcity, despite the printed commitment to the tautologous definition: the tenor of the argument on p. 209, *op. cit.*, seems to suggest this, although it follows upon the formulation of the tautologous definition. None of the criticism advanced here should obscure that fact that Lancaster has handled his model with admirable expertise.

12. Our task is yet incomplete. Even the Stolper–Samuelson–Metzler–Lancaster formulation does not found the theorem completely and solely on the basic model. We must still make the restrictive assumption that $\eta_x > c$. We should, however, clearly want to go the whole way and remove all restrictive assumptions and restate the theorem to include the entire matrix of possibilities: such that the theorem is logically true, given only the basic Stolper–Samuelson model. This formulation is: [1]

> *Protection (prohibitive or otherwise) will raise, reduce or leave unchanged the real wage of the factor intensively employed in the production of a good according as protection raises, lowers or leaves unchanged the internal relative price of that good.*

This is really the fundamental theorem that Stolper and Samuelson contributed to our knowedge of the properties of the basic model they were using. Given the basic model, our formulation is logically true for all possible cases.

13. It should perhaps be emphasised that the preceding analysis has been centred entirely on the problem of analysing the impact of protection on real wages of factors in the context of the basic model employed by Stolper and Samuelson. It should be possible, of course, to analyse the problem afresh in terms of models employing alternative assumptions. This, however, would be mostly destructive of the full validity of our theorem.

If we allow for complete specialisation with trade, for instance, we can claim only that protection will raise, lower or leave unchanged the real wage of the factor in which the *exportable* good is postulated to be intensive according as protection raises, lowers or leaves unchanged the internal relative price of the exportable good. But we cannot extend the theorem to the factor postulated to be used intensively in the production, if any, of the importable good because any increase in the internal relative price of the exportable good after complete specialisation must raise the real wage of *both* factors.

However, if we allow the optimum factor-proportions within industries, at given factor price-ratios, to change with scale, our theorem will continue to be logically true and the real wage of the factor intensively employed in a good will rise, fall or be unchanged according as the internal relative price of that good rises, falls or is unchanged with the imposition of protection.[2]

[1] This formulation stems directly from argument (2), which is founded exclusively, as the reader will remember, on the basic Stolper–Samuelson model.

[2] An apparent exception to this proposition may be investigated. Where the optimum factor-ratio changes with scale, at given factor price-ratios, it may happen, for instance, that if the production of labour-intensive importables expands, a higher proportion of labour is released than is needed in import-substitution, even though importables are *on average* more labour-intensive. In this case, increase in the production of importables will lead to a *rise* in the labour-to-land ratios, and hence *reduce* the real wage of labour. This case, however, does not constitute an exception to our theorem, because such technology involves a concave production frontier, so that increase in the production of importables occurs when the price of importables *falls* (and not rises). Hence the logical truth of our proposition, even when we allow for changing optimum factor-ratios with scale.

On the other hand, if we allow for changing returns to scale in either or both of the two activities, clearly it becomes impossible to maintain that our theorem will be logically true.

II. Protection and Real Incomes

14. Our analysis has so far been concerned with the original Stolper–Samuelson problem of discovering the impact of protection on the *real wage* earned by factors in employment. It seems useful, however, to emphasise that if we are interested in finding out the net change in the *real income* of the factors it is only in the case of a prohibitive tariff that a complete identity obtains between change in real wage and change in the real income of a factor. Where the tariff is non-prohibitive, the complication arises from the revenue earned by the Government. If this revenue is assumed to be redistributed to the owners of factors according to some formula, factors will derive incomes *both* from the real wage in employment and from the redistributed proceeds of the tariff-revenue.

Hence arises the interesting possibility that the factor whose real wage has been damaged by protection may still find its real income improved if the formula for the redistribution of the tariff-revenue is heavily biased in its favour. Since this possibility constitutes a qualification to the generally accepted implication of the Stolper–Samuelson analysis, it should be of some interest to delimit the conditions under which it may occur.

To begin with, this possibility of over-compensating the damaged factor *from the tariff-revenue* clearly cannot arise unless the real income of the country as a whole is improved by protection. We know from the preceding analysis that where the real wage of one factor is reduced, that of the other necessarily rises; hence, if protection did not bring some gain to the country as a whole, it should be impossible to overcompensate the factor with the damaged real wage (from tariff-revenues). To rephrase the proposition, then, accrual of gain to the protecting country from the imposition of protection is a necessary, though not sufficient, condition for the possibility of overcompensating the factor with the damaged real wage.[1]

In the following brief analysis we seek to relate this proposition to Metzler's formula for determining the impact of protection on the internal commodity price-ratio: partly to establish link with Metzler's pioneering analysis in this field and largely because it enables us to define, and distinguish between, situations in which the factor with the damaged real wage will be export-intensive (intensively used in exportables) and those where it will be import-intensive. The discussion is then briefly extended to the

[1] That is to say, whereas the country must have gained from protection before the damaged factor can be overcompensated from the tariff-revenues (necessary condition), this gain must be large enough to permit overcompensation (sufficient condition).

case where the initial situation is that of a tariff instead of free-trade and the effect of an *increase* in protection is the subject of inquiry.

15. In Fig. 2 let O_b be the foreign reciprocal demand curve facing country A. F is the free-trade point, OF yielding the corresponding terms of trade. I_a' is the trade-indifference curve of A passing through F at a tangent to OF and intersecting O_b at U. Its postulated curvature derives from

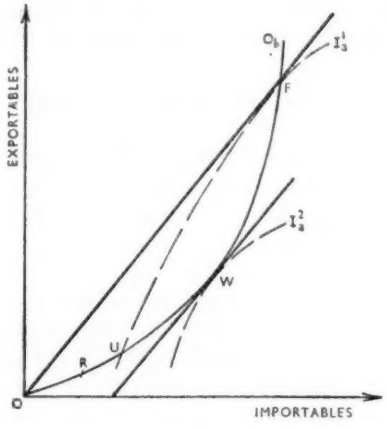


FIG. 2

the assumption of strict convexity of the production frontier and community indifference curves.

(1) Assume that the tariff-added offer curve of country A intersects O_b at U. The internal relative price of the importable good is then given by the slope of the trade-indifference curve I_a' at U, which is clearly, by virtue of the postulated curvature of I_a', greater than at F. We can deduce, therefore, that protection can leave the real income of the country unchanged only if the internal relative price of the importable good rises from the free-trade level with the imposition of protection (in turn, only if $\eta_x > c$).

(2) Similarly, by considering points on O_b to the left of U such as R, we can argue that protection can reduce the real income of the country only if the internal relative price of the importable good rises with protection (in turn, only if $\eta_x > c$).

(3) However, protection can increase the real income of the country whether the internal relative price of the importable good rises, is unchanged (W) or falls with the imposition of protection (in turn, whether $\eta_x \gtreqless c$).

Thus, where $\eta_x \leqq c$, the real income of the country will necessarily improve with the imposition of a tariff; whereas if $\eta_x > c$, the real income may rise, fall or be unchanged.

16. Where the comparison is confined to the real income and real wage of the factors in an initial free-trade position and after the imposition of a tariff, we can then conclude as follows:

(1) the export-intensive factor will necessarily become better off and it may be possible to overcompensate the import-intensive factor if $\eta_x < c$; [1]

(2) neither factor will become worse off and at least one better off if $\eta_x = c$; [2] and

(3) the import-intensive factor will necessarily become better off and it may be possible to overcompensate the export-intensive factor if two conditions obtain: (i) $\eta_x > c$, and (ii) the tariff is small enough to yield some gain to the country.

If we assume that importables are not inferior goods in the protecting country, it is clear, then, that inelastic foreign demand ($\eta_x < 1$) is a necessary, though not a sufficient, condition for the emergence of the possibility of overcompensating the import-intensive factor. Where, however, foreign demand is elastic and importables are not inferior goods, the export-intensive factor will necessarily find its real wage reduced by protection; and, for the possibility of overcompensating it to arise, it will be necessary, though not sufficient, that the tariff be small enough to make the country better off than under free-trade.

17. If, however, we wish to compare the real incomes and wages of factors in an initial situation of a tariff and after *increase* in the tariff, the analysis must be somewhat modified.

To begin with, the Metzler formula must be altered so as to read: the internal relative price of the importable good will rise, be unchanged or fall according as $\eta_x \gtreqless \dfrac{1 - c'(1 + t)}{1 - c't}$ where t is the initial tariff-rate and $c' = (1 - c)$ is the domestic marginal propensity to consume importables.[3]

[1] When $\eta_x < c$ we know now that: (1) the internal relative price of the importable good falls, thus increasing the real wage of the export-intensive factor and reducing that of the import-intensive factor; and (2) the country must have become better off. Hence the proposition in the text.

[2] Where $\eta_x = c$, we know that: (1) the internal relative price of the importable good is unchanged, thus leaving unchanged the real wages of both factors; and (2) the real income of the country must increase. Hence the proposition in the text.

[3] This formula is derived from the following analysis, furnished by Professor Johnson and replacing my earlier, unsatisfactory attempt.

Symbols: Let p be the *external* terms of trade, measured as the price of the importable good over the price of the exportable good, t the initial tariff rate and $\pi = p(1 + t)$ the *internal* terms of trade. c' is the marginal propensity to consume importables, at the initial terms of trade. C is the initial

It will be seen that where the initial situation is that of free-trade, t will be zero and the formula will reduce to the well-known Metzler formula.

Secondly, the impact on the real income of the country will not bear the same relationship to the shifts in the internal commodity price-ratio as in the previous analysis with the free-trade initial situation. It can be demonstrated, by a geometrical argument analogous to that used earlier, that although the internal relative price of the importable good must still rise for the country to be as well off as prior to the increased tariff, both reduction and increase in the real income of the country are now consistent with any shift in this price.

Thirdly, arguing from the optimum tariff theory, we can claim that the real income of the country will improve with increased protection if two conditions obtain: (i) the pre-increase tariff rate is less than the optimum tariff rate $t < \dfrac{1}{\eta_x - 1}$; and (ii) *either* the post-increase tariff rate is also less than the optimum tariff rate *or*, if it exceeds the optimum tariff rate, it is small enough to leave some gain in real income to the country from the increase in tariff.

---

domestic consumption of importables, Y their production and $M \equiv C - Y$ the initial quantity of imports. $r_m = (\eta_x - 1) = \dfrac{p}{M} \cdot \dfrac{\delta Sm}{\delta p}$ ($Sm \equiv M$) is the elasticity of foreign supply of importables; and $R = tpM$ is the tariff revenue.

Analysis: The simplest approach to the problem is to assume the internal terms of trade to be constant and to investigate the excess demand for importables when t changes. If the excess demand is positive, the internal terms of trade will rise to restore equilibrium; if negative, they will fall: assuming, of course, that we have " well-behaved " substitution elasticities in production and consumption.

Now, $R = tpM = \dfrac{t}{1+t}\pi \cdot M$. With π constant, we have $\dfrac{dp}{dt} = -\dfrac{p}{(1+t)}$. The shift in the demand for imports due to the change in the tariff is then given by:

$$\frac{dC}{dt} = c'\frac{dR}{dt} = c'\pi\left\{\frac{1}{(1+t)^2}M + \frac{t}{(1+t)} \cdot \frac{dM}{dt}\right\}$$

The shift in the supply of imports is given by:

$$\frac{dM}{dp} \cdot \frac{dp}{dt} = \frac{M}{p} \cdot r_m \cdot \frac{dp}{dt} = -\frac{M}{(1+t)} \cdot r_m$$

The excess demand for importables is then given by:

$$\left(\frac{dC}{dt} - \frac{dM}{dt}\right)$$

$$= \frac{c'\pi}{(1+t)^2} \cdot M + \left\{\frac{c'\pi t}{(1+t)} - 1\right\}\left\{\frac{-M}{(1+t)}\right\} r_m$$

$$= \frac{M}{(1+t)}\{c'p + (1 - c'pt)r_m\}$$

Substituting $r_m = \eta_x - 1$, assuming p to be unity initially by choice of units and simplifying, we arrive at the formula that the excess demand for importables will be positive, zero or negative according as:

$$\eta_x \gtreqless \frac{1 - c'(1+t)}{1 - c't}$$

It will be seen that this formula reduces to the Metzler formula when t is zero.

These considerations lead to the following conclusions: [1]

(1) The export-intensive factor will necessarily become better off and it may be possible to overcompensate the import-intensive factor from *increased* tariff-revenues when three conditions obtain: (i) $\eta_x < \frac{1 - c'(1+t)}{1 - c't}$; (ii) $t < \frac{1}{\eta_x - 1}$; and (iii) *either* the post-increase tariff rate is also less than the optimum tariff rate *or*, if it exceeds the optimum tariff, it is still small enough to leave some gain in real income to the country from the increase in the tariff.

(2) The import-intensive factor will necessarily become better off and it may be possible to overcompensate the export-intensive factor from *increased* tariff-revenues when three conditions obtain: (i) $\eta_x > \frac{1 - c'(1+t)}{1 - c't}$; (ii) $t < \frac{1}{\eta_x - 1}$; and (iii) *either* the post-increase tariff rate is also less than the optimum tariff *or*, if it exceeds the optimum tariff, it is still small enough to leave some gain in real income to the country from the increase in protection.

(3) Where, however, $\eta_x = \frac{1 - c'(1+t)}{1 - c't}$, the real wage of neither factor changes with the increase in protection. It follows, therefore, that the real income of both factors will increase, decrease or remain unchanged according as the increase in tariff raises, lowers or leaves unchanged the real income of the country: assuming, of course, that the tariff-revenues are divided among the factors in a given proportion.

18. In conclusion, it should be re-emphasised that the brief discussion presented here has been concerned only with the limited task of exploring some of the implications of the proposition that accrual of gain to the protecting country from the imposition of protection is a necessary, though not sufficient, condition for the emergence of the possibility of overcompensating, from tariff-revenues, the factor with the damaged real wage. It is planned to present a rigorous analysis of the sufficient conditions for the emergence of this possibility in a subsequent article.[2]

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[1] The first two propositions that follow assume that the factor stated to become necessarily better off continues to receive *at least* the same revenue as in the initial situation; this assumption being made explicit by the use of the phrase " from *increased* tariff-revenues." This assumption is needed because otherwise improvement merely in the real wage of a factor due to increased protection could be offset by an accompanying unfavourable distribution of tariff-revenues to the factor after the increase in the tariff.

[2] This calls for a rigorous analysis of distribution and demand, so that factor earnings and income subsidies from tariff-revenues could be related to real incomes of factors. Such analysis would preclude us from taking as given, as we have done here, the set of community indifference curves: this practice has been adopted in the present paper for strictly pedagogic reasons.

DUESENBERRY ON GROWTH AND FLUCTUATIONS

I

In the theoretical models of long-run growth in mature capitalist economies which have proliferated in recent years, two different streams of thought may be distinguished, going back to two different sources. The first of these sources is the theory of distribution. Models such as those of Mrs. Robinson and Mr. Kaldor [1] hark back to the classical economists and to Marx in relating the analysis of growth closely to the analysis of the forces determining the distribution of income between labour and capital. The relation between distribution and growth in these models arises partly from the mutual interdependence of the rate of profit and the pace of capital accumulation and partly from the effect of the distribution of income on the proportion of income saved. The other source is the theory of the trade cycle. It is in trade-cycle theory that the growth models of Dr. Goodwin and Professor Smithies,[2] for example, have their origins. The relation between trade-cycle theory and growth theory is that trade-cycle theory is concerned with what determines the level of demand in the short period, and this provides a possible approach to the explanation of what determines the long-run movement of income on the demand side.

The difference between these two approaches is partly a matter of substance and partly a matter of emphasis and selection—of what one chooses to explain by the model and what one treats as extraneous to it. Thus growth models originating in trade-cycle theory tend to treat as exogenously determined the " normal " (in the Marshallian sense) rate of profit on capital, whereas in distribution-oriented models the possibility of systematic long-run changes in the profit rate are a central theme. On the other hand, distribution-oriented theories commonly fail to give any very explicit account of the way in which fluctuations occur around the growth path, whereas models developed from analysis of the cycle are naturally more exact on this point.

The model of economic growth put forward by Professor Duesenberry in his important and distinguished new book [3] belongs to the second of the

[1] Joan Robinson, *The Accumulation of Capital* (1956); N. Kaldor, " Alternative Theories of Distribution," *Review of Economic Studies*, 1955–56, pp. 83–100, and " A Model of Economic Growth," Economic Journal, 1957, pp. 591–624.

[2] R. M. Goodwin, " The Problem of Trend and Cycle," *Yorkshire Bulletin of Economic and Social Research*, 1953, pp. 89–97, and " A Model of Cyclical Growth," in *The Business Cycle in the Post-War World*, edited by E. Lundberg (1955), pp. 203–21; A. Smithies, " Economic Fluctuations and Growth," *Econometrica*, 1957, pp. 1–52.

[3] J. S. Duesenberry, *Business Cycles and Economic Growth* (New York: McGraw-Hill, 1958. Pp. xi + 341. 50*s*. 6*d*.).

two types referred to. It springs from trade-cycle theory in that the concepts and relationships it uses are primarily those that have figured in trade-cycle theory rather than in the theory of distribution. But unlike most growth models that spring from this source, Duesenberry's model does not in its basic form yield a system of growth and fluctuations. Instead it yields a system of steady growth. Fluctuations are held to be due to causes extraneous to the main process of growth and are separately explained. This is the first distinctive feature of the model. The second is its treatment of the relative importance of demand and supply. A central topic in any growth model is the relation between the rate of growth of demand and the rate of growth permitted by population increase and technical progress—Harrod's natural rate of growth. By what mechanism, if any, are these two brought into alignment? Duesenberry takes his stand firmly on the side of demand as the prime determinant of the rate of growth. In normal circumstances factor limitations are held not to impose any significant constraint. Either there are plenty of resources or else the natural rate of growth adjusts itself to the requirements of demand.

In what follows we shall be concerned with discussion and criticism of the main outline of Duesenberry's model of growth (Sections II–IV, VI) and of fluctuations (Section V). In the course of building up this model Duesenberry provides much closely packed, original and at times controversial analysis of such topics as oligopoly, the effect of uncertainty on investment decisions, the supply of finance, investment in residential construction and the labour market. This analysis is conducted mainly in *a priori* terms, but with frequent supporting reference to empirical data on the American economy. Interesting as this material is and noteworthy as a contribution to the study of the topics concerned, large parts of it are not essential to the main purposes of the model, and will therefore not here be discussed.

The prime aim of the model is to explain the historical experience of the United States since the Civil War. The analysis is therefore conducted almost exclusively in terms of a closed economy. Moreover, the economic activities of government are not brought into the main model, though some indication is given of the way in which they will impinge in theory and have impinged in practice during the past twenty years.

II

We will begin by summarising the model.

The level of investment in any firm, and hence in the economy, is determined by the intersection of a marginal efficiency of investment schedule and a supply of funds schedule. The marginal efficiency of investment schedule is based ultimately on the idea underlying the acceleration principle, viz., that the marginal efficiency of investment is an increasing function of income (Y) and a diminishing function of the stock of capital (K). How-

ever, this is interpreted with a good deal of qualification to take account of uncertainty, indivisibilities, oligopoly, special factors affecting housing and so on. Moreover, it is held that even in the fairly short period there is a good deal of scope for varying the capital intensity of production, and this causes the marginal efficiency of investment schedule corresponding to a given level of income and capital stock to be fairly elastic.

The supply of funds schedule facing a firm is strongly affected by the imperfection of the capital market and by the principle of increasing risk. As a result, it is relatively inelastic, at least over certain ranges, and this serves to lessen the effect on investment of a shift in the marginal efficiency of investment schedule. Of the various sources of funds, internal finance has the lowest opportunity cost, up to the point where shareholders become restive; borrowing is the next cheapest, up to the point where the principle of increasing risk becomes important—where this point is depending on the size of the firm's initial debt in relation to its own capital, and also on the riskiness of the particular industry concerned; finally, there is the issue of equities, the cost of which, as measured by earnings yields, is the highest of the three. Duesenberry does not give any extended treatment to the possibility of systematic shifts over time in the forces affecting the supply of funds schedule, such as changes in firms' views as to the acceptable rate of return on their own funds, or as to the acceptable ratio of retained to total profits. In this sense the theory remains within a short-period framework and treats as given many of the considerations relevant to a long-period theory of distribution.

However, since the supply of funds is affected by the level of profits, some theory of distribution is needed, and this is duly provided. It is essentially a short-period theory, based on the doctrine that profits' share in income will rise if there is an increase in the degree of utilisation of capital (Y/K), (*a*) because the relative share of overhead factors, such as salaries, pensions and debt interest, if any, will fall, (*b*) because mark-ups over direct cost will tend to increase.[1] (The actual size of profits' share in income corresponding to any given degree of utilisation will be affected by the extent of competition as well as by the structure of cost.[2]) The degree of utilisation itself is determined by the working of the system as a whole.

The investment function that is yielded by bringing together all these considerations is in the first instance quite complicated. On the marginal efficiency side, investment is a function of Y and K (houses and other capital being entered separately), and on the supply of funds side it is a function of a number of variables, including profits, debt, depreciation allowances and retained earnings. However, most of the financial variables turn out to be themselves functions of profits, past or present, and profits are a function of Y and K (since profits' share in income depends on the degree of utilisation).

[1] Duesenberry, pp. 102, 261.
[2] *Ibid.*, p. 216.

So by a certain amount of simplication and by assuming linear relationships we arrive at the following familiar-looking equation

$$I_t = \alpha Y_{t-1} + \beta K_{t-1} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Subscripts denote time-periods ($K_{t-1} = K$ at *end* of period $t - 1$). α and β are coefficients of a catch-all character whose numerical value will be determined by the parameters in the marginal efficiency and supply of funds schedules. α will be positive and β negative.

This equation looks like no more than the generalised acceleration principle, and so indeed it is formally. But its interpretation is rather different. In the pure acceleration principle α in the above equation would represent the capital–output ratio aimed at by entrepreneurs and β would be minus unity: the postulate being that investment adds such an amount to the initial capital stock as will bring the capital–output ratio to the desired level (on the assumption of no change in income). In more generalised versions of the acceleration principle [1] account is taken of expectations, indivisibilities, time-lags and other such complications, so that although I_t is still a function of Y_{t-1} and of K_{t-1}, the parameters α and β are affected by a wider range of influences, and are not necessarily equal to the aimed-at capital–output ratio and -1 respectively. In Duesenberry's model the relationship is affected not only by these complications but also by the supply of funds. Thus because funds are in inelastic supply to the firm, the coefficient α, which measures the amount by which investment will change in consequence of a given change in income in the previous period (given the initial capital stock), will be much smaller than it would be in a pure acceleration principle model: a large rise in income may greatly raise the marginal efficiency of investment, but the amount by which investment can actually rise will be severely restricted by the supply of funds. (One result of this is that an initial deficiency of capital may take quite a long time to make up, and any minor shock-induced recession will therefore be cushioned by the backlog of investment opportunities.) This is partially offset by the effect of an increase in income in raising profits, and hence the availability of internal finance—this effect is held to contribute substantially to the value of α—but none the less α is expected to be well below unity.

The above investment function is combined with a fairly straightforward consumption function. Personal consumption out of disposable income is held to be a function of lagged income and lagged consumption, the latter to reflect the inertia of consumption habits, but in the first approximation this is simplified by omitting the lagged consumption term. Consumption is also affected by the distribution of income, since the corporate propensity to retain earnings is higher than the personal propensity to save and is affected by different considerations. Since the distribution of income

[1] Cf. for example H. B. Chenery, " Overcapacity and the Acceleration Principle," *Econometrica*, 1952, pp. 1–28.

depends upon the degree of capital utilisation, the level of consumption can be given by an equation similar in form to the investment equation above, namely

$$C_t = aY_{t-1} + bK_{t-1} \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The parameters a and b here are catch-all coefficients measuring the net effect of Y_{t-1} and K_{t-1} on C_t through the several channels mentioned. The parameter a may be thought of as the marginal propensity to consume out of the previous period's income that applies if K is constant. Both a and b are positive. (The higher K_{t-1}, given Y_{t-1}, the smaller the share of profits in income, and hence the smaller the proportion of income saved.)

Further to these two equations we have the identities

$$Y_t = C_t + I_t \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

and

$$K_t = (1 - k)K_{t-1} + I_t. \quad . \quad . \quad . \quad . \quad . \quad (4)$$

(k = the proportion of capital stock disappearing through capital consumption in each period.)

Substitution between these four equations leads to the following

$$Y_t = [(\alpha + a) + (\beta + 1 - k)]Y_{t-1} + [\alpha(\beta + b) - (\alpha + a)(\beta + 1 - k)]Y_{t-2} \quad . \quad . \quad . \quad . \quad . \quad (5)$$

This equation describes the path of income through time. It stands at the centre of the whole model. It involves certain important simplifications in that it neglects various lags, does not distinguish residential construction from other investment and is cast in aggregative rather than sectoral form, and these simplifications can be removed one at a time at the appropriate stage of the argument. However, the general working of the model can be sufficiently seen from the equation.

A second-order difference equation of this general form is what is used in the models of Hicks and others to yield not growth but a cycle. But, as is well known, this form of equation may yield several totally different patterns of behaviour according to the values of the parameters. One range of values will lead to damped or anti-damped cycles around a constant average value (cycles around a rising trend are not a possibility); another to convergence to stationary equilibrium; and another to explosive growth at what ultimately becomes an exponential rate. In the latter case the growth rate is stable in response to shocks in the upward direction; in response to downward shocks it is stable so long as the shocks do not exceed a certain crucial magnitude, but if they do exceed this magnitude the explosive growth will turn into explosive contraction.

The essence of Duesenberry's theory is that the parameters have in fact been within the range that leads to exponential growth and that the process so described has lain at the heart of the economic expansion actually ex-

perienced in the United States.[1] The exact values of the parameters have been what determined the rate of growth attained.

The possibility of deriving a model of steady growth from a second-order difference equation had not entirely escaped previous writers.[2] These writers encountered the following snag, however. In models based on the acceleration principle, if the parameters are such as to yield growth rather than cycles or convergence, the rate of growth per period tends to come out quite unrealistically high in relation to any reasonable definition of a "period" in terms of calendar time. In some models it is actually impossible to get moderate growth,[3] with others it can result only from very unlikely values of the acceleration coefficient or the propensity to save. Duesenberry's model avoids this difficulty because its assumptions are more complicated. Investment is determined not only by the forces described by the acceleration principle but also by the supply of funds. As a result α and β will be much lower than in an unqualified acceleration-principle model. Moreover, the term b in his model—which is absent from simpler models—helps to avoid cyclical solutions and so enlarges the range of possible rates of exponential growth. Hence it is possible with reasonable values of the parameters to get a solution which shows a rate of growth that is not grossly excessive in relation to historical experience and to what is physically possible.

III

It will be observed that equation (5) does not contain any term reflecting population growth or technical progress. The question arises, therefore, what forces, if any, bring the rate of growth that emerges from the model into line with the natural rate of growth. In particular, what happens if the rate of growth yielded by the model exceeds the natural rate of growth, so that growth is obstructed by shortage of resources? Duesenberry makes several points in answer to this.

(1) He does not think it likely that the parameters will be such as to

[1] Whether the process of growth in other capitalist countries has been of basically the same nature as in the United States is an issue not explicitly discussed. The contention would presumably be that it has in very broad terms, but with rather substantial qualifications due to the larger role of international trade.

[2] Cf. S. S. Alexander "The Accelerator as a Generator of Steady Growth," *Quarterly Journal of Economics*, 1949, pp. 174–97.

[3] Consider, for example, the very simple model where

$$I_t = v(Y_{t-1} - Y_{t-2})$$

and

$$Y_t = \frac{1}{s} I_t$$

so that

$$Y_t = \frac{v}{s}(Y_{t-1} - Y_{t-2}).$$

Here if v/s is less than 4 in numerical value the system yields fluctuations without growth; if, on the other hand, v/s exceeds 4, it yields growth at not less than 100% per period.

yield a very high rate of growth. This point will be reverted to in Section IV below.

(2) Population growth and technical progress will indirectly affect the rate of growth of the system in so far as they affect the size of the parameters. This they are likely to do in a number of ways, in particular: (*a*) technical progress in the form of new industries will tend because of indivisibilities to increase the extent to which capacity is built ahead of demand and so will raise investment—this is held to have contributed significantly to the growth of the United States economy; (*b*) population growth by lowering the rate of growth of real income per head for any given rate of growth of total income will raise the consumption–income ratio and will also raise the proportion of consumption expenditure devoted to the capital-intensive item housing. These and certain other similar influences tend in the direction of accommodating the rate of growth yielded by the model to the natural rate of growth. But the relation is indirect and there is no necessary tendency for a given change in the natural rate of growth to lead to an equal change in the actual rate of growth. It might be much more or much less.

(3) The natural rate of growth is itself flexible to some degree. If the economy is pressing against the limit of resources, the supply of resources will tend to respond in one or other of the following ways: (*a*) immigration will be encouraged; (*b*) labour force participation will be increased (this has a long-period aspect as well as a short-period one, *e.g.*, labour shortage may contribute to a gradual change in institutions and attitudes affecting the employment of women); (*c*) labour will be encouraged to move from low-productivity sectors, especially agriculture, to high-productivity sectors, since frictions in the labour market inhibit this movement unless there is excess demand for labour in the high-productivity sectors; (*d*) employers may be stimulated to improve efficiency, especially in low-productivity sectors. Some of these represent a permanent increase in the natural rate of growth; others a once-for-all increase in the supply of resources. But even if the increase is once-for-all it may take many years to exhaust if it is a large one and if the excess of the actual rate of growth over the natural rate of growth in the initial situation was not large.

What is being contended under (3) is thus that the natural rate of growth is not a single path as in Diagram 1 but rather a band as in Diagram 2. An actual rate of growth falling anywhere within the band can be accommodated without a manifest increase or decrease in unemployment. Under (2) it is contended that if the band is steeply inclined upwards this will have some tendency to cause the actual rate of growth also to be high. Under (1) the contention is that the parameters are not likely to yield a rate of growth greatly in excess of the upper limit of the band.

Finally, however, it is admitted that notwithstanding these considerations it could happen, at least in principle, that the system tended to yield a rate of growth higher than the maximum physically possible. In this event

Duesenberry maintains that the system would bounce off the ceiling in a series of *mild* cycles, as shown in Diagram 3. It will be noted that apart from the mildness of the cycles, on which see below, pp. 761–3, this result is substantially identical to that of the Goodwin model. Duesenberry does not, however, regard this as a really likely or important contingency. If admitted as the typical case it would, of course, imply a very radical change

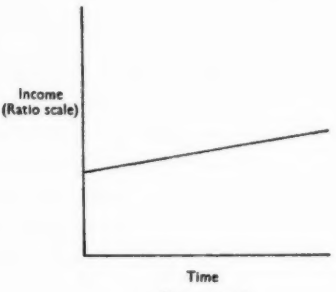

DIAGRAM 1

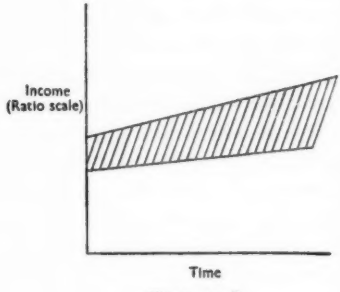

DIAGRAM 2

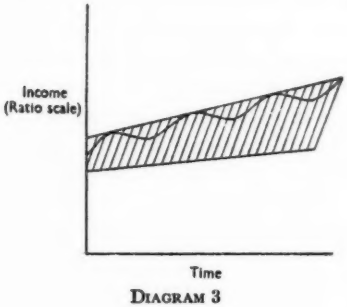

DIAGRAM 3

in the interpretation of the system's working, since the average rate of growth actually achieved would depend not on the parameters in the income-generation equation, as in the normal case of Duesenberry's model, but on the maximum value of the natural rate of growth.

IV

We come now to the main and most obvious criticism of this model of growth.

As we have seen, because the model is not based merely on the acceleration principle, it is not open to the objection that plausible values of the parameters are incapable of yielding moderate growth. (Indeed, because of the multiplicity of forces affecting the parameters it is not easy to say anything very definite about what *are* plausible values of the parameters.) But granted that there is no inherent reason why the parameters should not

yield moderate growth, it does not follow that they actually will. For the model to provide an acceptable account of reality, the parameters must fall within a certain range, so as to avoid, on the one hand, non-growth solutions and, on the other hand, growth at an excessive rate. This is *prima facie* rather restrictive and lays the model open to the objection that the parameters are unlikely to have stayed around the same level for the space of a hundred years or so. This is similar to the objection brought against linear cycle models that unless recourse is had to shocks they can explain the persistence of cycles of roughly constant amplitude only if the parameters have a certain unique value. In the present case the parameters do not have to have a unique value, so the objection is less strong; they merely have to fall within a certain range. The force of the objection therefore turns very much on how wide this range can in fact be without giving unacceptable results.

Whatever the answer to this may be, it is certainly a gap in Duesenberry's exposition that he does not at any stage come to grips with this problem. The reasons he puts forward at various points for doubting that the rate of growth is likely to be " very high " are practically all put in non-quantitative form. No numerical examples are given to show the orders of magnitude that may be involved, either in the working of the main equation or in the working of the various lags and qualifications it is subject to. The same applies to the treatment given to the effect of structural changes; ingenious and interesting as much of it is, it is essentially non-quantitative.

The table annexed shows the results of some simple arithmetical experiments done to test the issue. What we do here is to hold constant the values of four of the five parameters in equation (5) and then see how widely the fifth parameter α, which measures the responsiveness of I_t to a change in Y_{t-1}, is capable of varying without giving unacceptable results. Values are chosen for the four parameters a, b, β, and k which appear reasonable in themselves and not such as actually to preclude moderate growth. If moderate growth is to result, the lower limit of the rate of growth (r) is clearly zero. Where we place the upper limit—the limit beyond which growth is excessive—depends on the duration of the " period " in calendar time. This is an empirical matter, depending on the duration of the relevant reaction lags, etc. Duesenberry himself reckons that the " period " will be in the region of six months. On this basis a value of r of 0·05,

a	β	b	k	α		
				$r = 0{\cdot}0$	$r = 0{\cdot}05$	$r = 0{\cdot}1$
0·75	−0·1	0·05	0·1	0·333	0·375	0·420
0·8	−0·1	0·05	0·1	0·267	0·313	0·360
0·8	−0·2	0·05	0·1	0·400	0·438	0·480
0·8	−0·2	0·1	0·1	0·300	0·350	0·400
0·8	−0·2	0·1	0·2	0·267	0·321	0·375

representing an annual rate of growth of slightly over 10%, may perhaps be taken as a reasonable maximum. But to give the benefit of the doubt, the table also shows the values of α yielding $r = 0{\cdot}1$, which would represent an annual rate of growth of 21% on the basis of a six-month period.

This table does appear to suggest that there is only a fairly narrow range within which α is free to vary, given the other parameters. It also illustrates the sensitivity of the rate of growth to changes in the other parameters: *e.g.*, if $\alpha = 0{\cdot}333$, $\beta = -0{\cdot}1$, $b = 0{\cdot}05$ and $k = 0{\cdot}1$, then the table shows that if the marginal propensity to consume $a = 0{\cdot}75$, r is zero, while if $a = 0{\cdot}8$, r is between 0·05 and 0·1 (0·07 approximately).

This is a test in terms of *structural stability*—the ability of the model to withstand changes in its parameters without its performance undergoing a major transformation. The result is not very favourable. But of course a test in these terms is by no means decisive (even apart from the obvious fact that the table given covers only a certain range of values of a, β, b and k). In the first place, a change in circumstances which alters one parameter is quite likely to alter one or more of the others as well, and also perhaps the natural rate of growth. This may work in such a way as to help. In the second place, equation (5) is only a first approximation, and needs to be qualified as a description of reality, particularly by certain additional lags. These lags have the effect of damping down the rate of growth, and they operate more strongly the higher the rate of growth tends to be.[1] They thus serve to raise the value of α yielding the maximum plausible rate of growth, while also raising, but to a less extent, the value of α yielding the minimum rate of growth; the range of values of α that will yield acceptable results is thus enlarged. In assessing these limitations on the test, however, note must be taken of the ease with which the unqualified model can involve absurdly high rates of growth or else fail to yield growth at all. If the qualifications referred to are to endow the system with an adequate degree of structural stability, their effect must be pretty strong.

Duesenberry is aware that his model implies rather serious restrictions on the values of the parameters. (He appears to be more concerned with the possibility of non-growth solutions than with the possibility of growth at an excessive rate.) The reply he offers carries conviction only on the assumption that no alternative type of growth model can be devised. He writes (pp. 238–9):

> "It may be said that it is a remarkable coincidence that out of all conceivable savings functions, profits functions, and so on, the ones which came into existence in the nineteenth century were so well adapted to the rates of population growth, development of new industry, and other characteristics of that century. One can only say that, if they had not been, capitalism would never have survived so long."

[1] Duesenberry, pp. 211, 278.

V

We come now to the theory of fluctuations. Duesenberry suggests that there are in principle two quite different ways in which depressions could occur. In the first place, the parameters of the system might temporarily or permanently become such as to yield a non-growth solution. Recovery from this sort of depression would be problematical. In the second place, depressions might come about from causes such as speculation and financial crises that are extraneous to the main dynamics of growth. Duesenberry maintains that all depressions in the United States since the Civil War, with the possible exception of the 1930s, have been of the second type. He thus reverts to a rather old-fashioned kind of view and denies that any form of the multiplier–accelerator interaction (referred to as the capital-adjustment process) has been responsible for historical fluctuations. This contention is based both on empirical observation of certain characteristics of past depressions—notably the absence of regular periodicity—and on *a priori* reasoning as to the unlikelihood of major depressions resulting from the normal process of growth and capital accumulation, so long as the underlying parameters are such as to permit growth at all.

It may be noted that some such theory of fluctuations is called for by the theory of growth discussed above, since if the capital-adjustment process is to be used to explain growth it cannot also explain fluctuations. The converse does not apply: it would be possible to have this theory of fluctuations, but some different theory of growth.

Duesenberry does not deny that capital adjustment elements play some part in fluctuations and act as a destabiliser. What he does deny is that these elements have been or could be by themselves responsible for major depressions. He also denies that they provide a satisfactory explanation of turning points.

We may consider first the upper turning point. Any realistic treatment of historical fluctuations must give a good deal of emphasis to shocks and disturbances that are random from the capital-adjustment point of view. Duesenberry's brief sketch of the history of fluctuations in the United States since the Civil War performs a valuable service in showing the great diversity of the various depressions within this period, both in severity and in apparent causes. The misleadingness of any purely mechanical model as an account of reality is plain. The question remains, however, whether the tendency for the stock of capital to rise faster than the level of output, which is the basic cause of the downturn in capital adjustment models and which Duesenberry denies, has not in some form or other been present at a sufficiently large proportion of upper turning-points for it to rank as a systematic element in the explanation of the downturn, even though it has sometimes been heavily overlaid with other factors and even though the tendency to fluctuations from this cause has in certain periods been largely

damped out. Duesenberry's contention appears to be (p. 244) that only a small percentage change in the ratio of capital to output can be brought about by the investment of a single year or two, and that such a small change could not initiate such sharp reversals as have been experienced. But it is surely impossible to say *a priori* how large a change in the capital–output ratio is needed to cause a sharp reversal. It is marginal adjustments that matter, and changes which are not large when averaged over the economy as a whole may substantially affect the inducement to invest. It is true that many if not most depressions have been triggered off by spectacular events in the financial sphere rather than by any manifest tendency for capital accumulation to outstrip the rate of growth of income. But it is always open to question whether the crash was not a reflection of a more deep-seated tendency to capital saturation. This is recognised by Duesenberry, and he also concedes that the conditions needed for a financial shock to cause a severe contraction will not normally be satisfied until some years have elapsed since the last one, thus producing a limited element of regularity in timing. In other circumstances shocks may occur without causing any serious general contraction.

With regard to the interpretation of individual upper turning-points, it may be observed that while there is nothing in the capital-adjustment theory of the cycle to imply that the downturn will be reached simultaneously in all industries, slumps caused by capital saturation in particular lines, *e.g.*, railway-building or house-building, are none the less often better regarded not as a manifestation of the capital-adjustment process but as the result of the exhaustion of a particular batch of investment opportunities, the size of which, in the short run at least, does not depend much on the level of income. The doctrine that slumps of this sort recur systematically is essentially Schumpeter's theory of the cycle. Duesenberry attaches a good deal of importance to non-maintainable bouts of investment in particular industries, but he treats them as falling into the category of shocks, and Schumpeter's views are not mentioned. Admission of a systematic cyclical tendency of the Schumpeterian type in innovatory investment or some classes of it would on the face of it strengthen Duesenberry's model rather than weaken it, since such cycles can without inconsistency be superimposed on an upward trend due to the capital-adjustment process.

Let us consider now the lower turning-point. The difficulties experienced by capital-adjustment models in explaining this phase of the cycle are well known, and Duesenberry states them very cogently. The crucial difficulty is the non-occurrence in all historical slumps except the 1930s of the negative net investment which the models appear to require. In Duesenberry's model, where the original downturn is explained as due to a shock, the difficulty of explaining the recovery is much less, for in the nature of things the shock is likely to reverse itself presently—*e.g.*, after a financial crisis, confidence in the banking system will presently be restored—and this

will provide a stimulus to recovery. This stimulus will be effective in actually bringing about a recovery so long as the cumulative forces of contraction have not in the meanwhile carried the system down too far. Duesenberry analyses with much ingenuity the conditions which on his model determine whether or not this will be the case.

It would be generally agreed that recessions caused by shocks will be terminated sooner and more easily than recessions caused by partial or general capital saturation.[1] But is the difficulty of recovery in the latter case so great that only the depression of the 1930s fits the description, as Duesenberry suggests? In facing this well-known crux, all one can do is to mention such factors as sectoral disinvestment, especially in inventories, sectoral floors and the less obvious form of negative net investment that consists of proceeding abnormally slowly with innovatory investment, and suggest that, when they are taken into account, it is not impossibly difficult to explain how recovery occurs from a capital-saturation slump without total net investment becoming negative.

An important implication of Duesenberry's theory of the cycle is that so long as the parameters are such as to permit growth there is no inherent *need* for the economy to suffer severe slumps. If speculation and financial disturbances can be avoided, steady growth is possible without cycles. This applies both to the past and to the future. For example, had there been stronger financial institutions and less speculation between the Civil War and 1914, slumps would then have been avoided. Growth and capital accumulation would, on the average, have proceeded rather faster than they did.[2] In the absence of slumps the rate of growth and the rate of capital accumulation would have been at most, say, 25% higher on the average than they actually were. Whether growth at this rate would have been possible without running into a ceiling depends on how much flexibility there was in the natural rate of growth. Flexibility of the order of 25% does not in the circumstances of that time appear an unreasonable postulate.

The consequences of impact with the ceiling have, however, a more general significance to the theory. As mentioned earlier, Duesenberry holds that if the growth of the system were at any time so rapid as to bring it up against an inflexible ceiling on the supply side, the result would be a mild recession rather than a severe one and that impact with the ceiling can therefore not serve as the explanation of the more severe depressions experienced in history. This conclusion appears to depend on a particular interpretation of the way the ceiling works, though this is not expressly stated. This interpretation is that impact with the ceiling has similar effects to an unfavourable but temporary shock. Impact with the ceiling

[1] Cf. R. A. Gordon, "Investment Behaviour and Business Cycles," *Review of Economics and Statistics*, 1955, pp. 23–34.

[2] Capital accumulation would not, however, have been quite so high as it was at the cyclical peaks, since speculation raises investment abnormally in the boom as well as depressing it in the slump.

tends to lessen the rate of growth of income, and thus may lead to actual contraction; but once sufficient slack in the system has reappeared the depressing influence of the ceiling will cease to be felt, and this will act as a positive stimulus—a *reversal* of the initial shock—which will carry the economy back into its normal growth path. Moreover, recovery can be relied upon to happen more promptly than after some other kinds of shock, since *ex hypothesi* the depressing influence of the ceiling will cease to be felt before the contraction in income has gone very far.

This is not quite the way in which the ceiling has been looked upon in most models of the cycle where it has been invoked. As a rule the ceiling is regarded as a constraint rather than as an actual deflationary shock—a cause of non-linearity in the investment schedule rather than of a downward shift in the schedule. Therefore, when the pressure of the ceiling ceases to be felt because income has fallen, it does not act as a positive stimulus. On this reckoning a ceiling-induced recession will not have any particular tendency to correct itself quickly. Suppose that the parameters of the system are such as to cause explosive growth, and that the system then in period t runs against the ceiling. The rate of growth is thereby reduced below what it would otherwise have been. This will serve to discourage investment in period $t + 1$, and as a result investment and income in that period may fall below the ceiling level. Suppose they do, and that, moreover, income in $t + 1$ is actually lower than in t.[1] Net investment in $t + 1$, however, remains positive. Assuming that investment (I) is a function of the levels of income (Y) and capital (K) in the preceding period, I_{t+2} must be lower than I_{t+1} since Y_{t+1} was lower than Y_t and K_{t+1} was higher than K_t. The falling away from the ceiling does not have any tendency to cause a rise in I between period $t + 1$ and period $t + 2$, since Y was already below the ceiling level in $t + 1$ and I_{t+1} was therefore unaffected by the ceiling (except insofar as the ceiling had served to hold down Y_t). A cumulative contraction therefore sets in. This may, of course, be held in check by such things as the arrears of investment left over from the boom, lagged reactions of some classes of investment, etc., but the distinctive feature of the early reversal of an adverse shock is absent and the slump is therefore liable to be prolonged.

In which of these two possible ways does the ceiling actually work, supposing it works at all? It is not particularly difficult to make a case for treating it as a shock subject to reversal as Duesenberry's argument requires. All that is needed is to postulate an appropriate lag or lags in its operation. Thus in the example above, although the ceiling is not actually in operation in the period $t + 1$, I_{t+1} may be held down by the ceiling because the ceiling was in operation when this investment was being decided on. I_{t+2}

[1] A downturn need not necessarily result from impact with the ceiling. Whether it does so depends on the values of the parameters. I have discussed this point more fully in "A Note on Crawling along the Ceiling," to be published shortly in *The Review of Economic Studies*.

may then represent a recovery. Equally, however, it is arguable that lags of this sort are not sufficiently important to have the effect required.

The suggestion that the operation of the ceiling is subject to lags in such a way that its removal acts as a positive stimulus is a distinct contribution to the theory of the lower turning-point. It is not, however, obviously more compelling than the various other explanations of recovery put forward in capital-adjustment models which Duesenberry rejects as unsatisfactory. It does not therefore establish convincingly that a ceiling-induced contraction will necessarily reverse itself more easily than a contraction due to some other shock, or indeed that it will reverse itself easily at all. The possibility of impact with the ceiling leading to a severe slump is not ruled out.

VI

In principle it is not difficult to devise means of testing alternative theories of the cycle. The large number and diversity of historical fluctuations may make generalisation difficult, but at least it affords plenty of material. There is not yet agreement on the conclusions, but it is reasonably clear how one may proceed. But when it comes to choosing between alternative models of long-run growth, the appropriate procedures and criteria are rather less plain. Different models involve different assumptions, especially assumptions about the stability of certain functional relationships, and they can be criticised if the required assumptions are not plausible. This is the nature of the criticism of Duesenberry's model put forward about on the score of structural stability. But to judge a theory by its assumptions is open to methodological objection,[1] and though this approach is not without its uses (especially in view of the difficulty of getting any decisive answer from empirical tests) it is unsatisfactory in many ways. After all it is essentially a matter of degree—the assumptions may be rather restrictive or implausible, but who is to say whether they are prohibitively so? The alternative procedure is to see how closely the results implied by the model conform to past experience and how successful they are in predicting the future.[2]

[1] Cf. M. Friedman, "The Methodology of Positive Economics," in *Essays in Positive Economics* (1953), pp. 4–43.

[2] A third approach which is sometimes used, explicitly or implicitly, is to judge a model by how much it explains and how much it treats as extraneous. But this is really an indication of the completeness of a model rather than of its validity. There is nothing inherently fallacious about the notion that the values of some important magnitudes—the rate and character of technical progress, the degree of monopoly, the rate of interest—are determined by forces that are essentially different from those that govern the behaviour of the economy when these are taken as given. A model-builder who takes this view may either have a separate theory as to how these magnitudes are determined or else he may treat them as unexplained constants, in which case his theory will be incomplete but not necessarily invalid within its limited field. Likewise there is no logical reason why growth and fluctuations should both be explained in terms of a single theory. The virtue of explaining everything important within the ambit of a single simple model is æsthetic rather than scientific.

These two aspects of empirical testing as applied to Duesenberry's model may be considered separately. First, the experience of the past. Which provides an apter picture of what has happened: Duesenberry's model or one in which population growth and technical progress are more directly responsible for the growth of demand, as they are in the models of a number of other recent writers? So long as the economy enjoys fairly steady growth, the outline result yielded by the two types of model will not be very different. Demand and productivity will be rising, the average proportion of the labour force unemployed will be relatively small and there will be no systematic reason for changes in the capital–output ratio, the proportion of income saved or the distribution of income. Neither type of model is therefore inconsistent with what Mr. Kaldor has called the " stylised facts " of economic growth, and more refined tests need to be applied to discriminate between them.

In some respects the reaction of the system to a change in circumstances may be more instructive than its behaviour when growing steadily. An example may be quoted which is interesting but which unfortunately serves to underline the difficulty of reaching a definite answer. Since the First World War the rate of growth of the adult population of the United States has been markedly lower than it was previously, largely because of the introduction of legal restrictions on immigration. If the rate of growth of income is unaffected by the supply of labour, as the simplest version of Duesenberry's model would imply, one would expect to find the fall in the rate of growth of population to be offset by a rise in the rate of growth of production per head. If, on the other hand, the ultimate determinant of the rate of growth of output is the rate of expansion of factor supply, one would expect there to be a fall in the rate of growth of total output, and there would be no particular presumption of an offsetting increase in the rate of productivity growth. In fact, productivity in the United States has risen decidedly faster since the First World War than it did before, though the change has not been quite enough to prevent a fall in the rate of growth of total output.[1] This would appear on the face of it to be a distinct point in favour of a demand-dominated theory of the Duesenberry type. But it is not really as simple as this. Duesenberry himself maintains that the underlying forces making for growth on the demand side were seriously weakened for various reasons after 1919. He would not therefore be able to attribute the speeding up of productivity growth to the persistence of the old underlying growth equation in face of a slowing down of population growth. He puts forward a number of special causes to explain the rapid growth of production in the 1920s (speculation in real estate, the stock-market boom, etc.), and since he does not suggest that the underlying forces making for growth have become significantly stronger again since then, he would

[1] See S. Fabricant, *Basic Facts on Productivity Change*, Occasional Paper 63, National Bureau of Economic Research (1959).

presumably also seek special explanations (such as government spending) for the vigorous growth of the 1950s. So his theory is not really much more successful in providing a built-in explanation of the speed-up of the rate of growth of productivity than the alternative type of theory is.

When it comes to prediction of the future, Duesenberry's model is capable of yielding spectacularly different conclusions from other types of model. For although his theory is optimistic in one respect, in that it denies the inevitability of cyclical depressions, in another respect it is pessimistic, in that it represents the whole growth process as highly precarious. An unfavourable change in the parameters, such as an increase in the relative importance of industries with low capital-intensity, is liable on his model to cause a complete breakdown of growth, not merely to cause growth to proceed with a rather lower average level of employment and/or profits, as in other models. In this it is akin to the stagnation thesis in its most extreme form. Unfortunately from the scientific point of view, though fortunately from others, this predictive test is not likely to be carried out, since governments now assume responsibility for preventing severe slumps. Indeed, the role of the Government in controlling the volume of demand, and also its composition to some extent, serves more generally to destroy the usefulness of predictive capacity as a test of growth models. Although many of the concepts and relationships involved are likely to retain their relevance to the contemporary scene, the devising and testing of self-contained models of capitalistic growth should perhaps be regarded as essentially a historical study, relating to an epoch now closed.

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REVIEWS

The Theory of Public Finance. By R. A. MUSGRAVE. (New York and London: McGraw-Hill, 1959. Pp. xvii + 628. 97*s*.)

MUSGRAVE's book has been long-awaited and long-heralded. Let it be said at the beginning that it will immediately take its place as one of the greatest works in its chosen field. In erudition, theoretical insight and encyclopædic knowledge it ranks with great treatises in other fields such as Schumpeter on *Business Cycles* or *The History of Economic Analysis*. It would seem that the author has been engaged on the work for twenty years; it is quite safe to predict that it will be widely read twenty years from now, and most probably a great deal longer.

Having paid tribute to the stature and intrinsic worth of the book, I should like to work off my irritation straightaway about a minor but infuriating defect. There is an extremely large number of misprints in the book, some of which are very minor (such as names of authors misquoted), but some of which do affect the sense of the argument. It is understood that many of these have now been spotted and are to be corrected in the second edition (and there certainly will be a second edition), but in the meantime it is quite impossible just to hand the book as it stands to any student. It is simply astonishing that an author so well versed in the marginal principle should have failed to apply it to himself. A very small additional effort at the final manuscript or galley-proof stage would have brought in vast gains in accuracy and clarity.

The title of this book is, for once, a close guide to the contents. Musgrave's approach is nakedly and unashamedly theoretical; he is concerned with both normative and positive aspects of public finance, but has no truck with such matters as institutional backgrounds, actual tax arrangements and so on. Part I (" Statement of Issues ") and Part II (" The Satisfaction of Public Wants ") are largely concerned with the normative side. Musgrave argues that the budget of the public sector has three branches—allocation, distribution and stabilisation. The most original feature of this part is the distinction between two functions of the allocation branch—the satisfaction of *social wants* (public goods which must be consumed in equal quantities by all) and *merit wants* (the conventional social/private discrepancies). A good deal of the subsequent argument turns on this distinction. The benefit and ability to pay theories are then critically examined; and both are rejected as being incomplete solutions to the determination of the budget plan. The fundamental defect of the benefit theory is characterised (p. 134) as being that it provides no way of ascertaining the true benefits; and in the ability to pay case there is no

visible means of ascertaining social preferences (p. 134). In the end, therefore, Musgrave argues that the provision of social wants, merit wants and the determination of the income distribution pattern (and also the issues of employment and price stability?) are matters to be resolved by voting through ballot boxes rather than voting through purses. At the normative level economics has something to say about avoiding excess burdens (Chapter 7) and the appropriate index of equality (Chapter 8) but precious little else. A final chapter in Part II is an interesting but somewhat incongruous excursion into national-income literature.

Part III ("Adjustments to Budget Policy: Classical Aspects") deals with the traditional positive problems: the relative effects of different tax and expenditure systems on work effort, saving, relative prices and outputs, investment and the distribution of income. Part IV ("Compensatory Finance") accounts for about one-third of the book. The author weaves into the narrative some discussion of growth problems and a lengthy discussion of the theory of debt policy as well as the subjects more usually covered by this heading.

This very brief summary may give some idea of the width and depth of the work. It may be most helpful now to use the remaining space to explore, first, some of the book's outstanding merits and, second, some of the sins of commission and omission.

The merit *par excellence* of this book is the high quality of the theoretical reasoning. Without recourse to anything more than elementary mathematics Musgrave succeeds in cutting through a vast amount of tangled top-growth and sorting out the main stems of argument clearly and concisely. Thus the excess burden of indirect taxation argument (pp. 140–55), the effects of various taxes on the division of personal income between consumption and saving (Chapter 12) and the ramifications of the balanced budget multiplier theorem (pp. 432 ff.) are all set out so explicitly that there is no longer any excuse for the old fallacies to crop up again. There are many innovations in the book as well as solutions to old problems: a good example is the insistence (*e.g.*, pp. 243–6) on considering group reactions as well as those of the representative individual. The essential point here is that if we substitute a progressive income tax for a proportional tax we must look at the relative changes in marginal and average rates for each income group and the marginal rates of substitution of leisure for income for each group separately if we are to have any hope of sorting out the answer.

It cannot be denied that the author's willingness, and indeed anxiety, to explore these theoretical issues fully leads him to consider many unusual and indeed queer cases (*e.g.*, the "57 varieties" of assumptions about the possible shapes of marginal utility of leisure and marginal utility of income curves in Chapter 11 and the manifold ways (Chapter 18) in which compensatory tax adjustments may influence consumption). Although one can have some reservations about the methods of treating these problem

children, there can be no question that it is extremely valuable to have the answers all on tape, as it were.

To have explored some of these issues thoroughly would have been no mean feat; to keep up the performance over 600 pages is an intellectual achievement of great distinction.

If there were nothing to disagree about in a book of this length, it would be a matter for condemnation of the author, or the reviewer, or both. The first general point is really one of methodology. As it stands, the book is likely to be found overwhelming by most readers. Case after case is considered, every possibility is explored, no stone left unturned; and so it is hardly surprising that one has no clear impression when one gets to the end of which things are important, which results are the more likely and so on. Fundamentally, I think, this springs from the author's predilection for abstract theoretical reasoning and his reluctance to cite empirical evidence. There is nothing wrong in being a tool-maker, especially when the maker does clearly understand, as in this case, that his tools are meant to be used. But it is surely reasonable to expect some indication about whether any particular implement is likely to be in more general use than others. Thus in Chapter 11 we have a long refined analysis about the effects of taxes on incentives to work followed by a rather feeble single page on secular evidence. There have been a number of recent inquiries on the effects of taxation on incentives to work; discussion of these findings, their merits and demerits, would surely have enriched the argument. Similarly in Chapter 12 the point is (quite properly) made that the substitution of a consumption tax for an income tax will be more favourable to saving, the more is saving for the purpose of accumulation than for future consumption. But there is no word on which is or is likely to be the more realistic case. Musgrave would, one feels, be a superlative general in a mopping-up campaign remorselessly rounding up pockets of enemy resistance, but perhaps not quite so outstanding leading the charge towards a single strong point.

The second point is one of arrangement. Obviously the book has grown piecemeal over the years and suffers some of the consequences. We have already remarked on some incongruity in Part II. Similarly, it is not entirely obvious why problems of growth should all be discussed in Part IV on Compensatory Finance. Nor are topics always co-ordinated as they should be. There is some overlapping discussion of effects of taxes on the work/leisure choice and on the consumption/saving case in Part II (Chapter 7) and Part III (Chapters 11 and 12). Capital-gains taxation, business-income taxation and depreciation provisions are all discussed at various stages. But it would need an extremely perspicacious reader to divine that these problems hang together and that, for instance, the inclusion of capital gains in the personal income tax net is to a large extent an alternative to the differential taxation of undistributed profits.

Thirdly, and perhaps inevitably, the book is unquestionably uneven. There is no clear distinction in logic between unevenness in the sense of spending too much time on one subject at the expense of another and unevenness in the sense of one page being more difficult than another. As the first is a matter of judgment and the second one of fact, one cannot be dogmatic on this general point. Nevertheless, it does seem odd to find some twenty pages on voting theory, drawing on, *e.g.*, Arrow and Black, but only four pages on Federal Finance without any reference to Wheare or similar authorities. There are twenty-four highly compressed pages on the effects of taxation on risk-bearing (Chapter 14) but no connected discussion at all on the principles of minimising the costs of administering taxes. A whole chapter (Chapter 15) deals with the complexities of price and output adjustments by business firms to various taxes, but the overlapping jurisdictions of income taxes across territorial frontiers and the problems of property taxes scarcely get a mention. But, to repeat, matters of opinion cannot readily be disentangled from matters of fact on such points as these.

Finally, there are points at which Musgrave's analysis seems dubious or obscure. This again is inevitable in a book of such proportions, but perhaps one example may be given of each. On pp. 249–50 the author argues that an equal yield proportional consumption tax is likely to induce a larger amount of work effort than a proportional income tax, principally because of group effects. It is argued that as the average propensity to spend rises with income, the imposition of a proportional spendings tax will be equivalent to substituting a regressive tax on income for a proportional tax, and that therefore work incentives will be greater. However, the problem is not one of the relation of marginal rates to average rates of spendings tax but rather of that of marginal spendings tax rates to marginal income tax rates. If one allows for the fact that the expenditure tax rate must be greater than the equivalent income tax rate (by a factor depending on the national savings ratio), the fact that tax on saving for future consumption is postponed rather than abolished and the further point that the marginal propensity to save is usually found to be fairly invariant to income, the argument becomes much more open than appears from the text.

As an example of obscurity, the discussion on p. 168 about the measurement of depreciation in real terms may be cited. Quite apart from the (obviously erroneous) transition from $ to million $ magnitudes and the inadequacy of the statement that in times of inflation " original cost does not permit the investor to recover the real cost of his outlay," the general argument in terms of real value of net worth is not likely to be found enlightening by many.

But compared with the intellectual achievement of incorporating all these ideas and arguments within two covers, these criticisms are of small

importance. The note on which one must end is that from now on anyone interested in the theory of public finance can simply be told, " It's all in Musgrave."

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Theory and History. By LUDWIG VON MISES. (London: Jonathan Cape, 1958. Pp. ix + 384. 30*s.*)

THIS new volume by Professor von Mises has the same texture and tone as his earlier books on *Human Action* and *Socialism.* There are frequent cross-references, and both the methods of argument and the conclusions are perfectly consistent. The defence of the liberal market economy and of economics and " praxeology " as *wertfrei* intellectual studies with " unique epistemological character " will be familiar to all his readers. The defence, of course, is most frequently a scathing attack. Never can the market economy have been handled so stridently or the history of ideas so brusquely. The book is littered with adjectives such as " absurd," " preposterous," and " pernicious," and nouns such as " pseudo-economics," " fantasy " and " slavery." Professor von Mises attacks Marx for refusing to argue with his opponents and assuming that it was sufficient to unmask their bourgeois background and thereby the necessarily " idealogical " character of their doctrines. Nonetheless, he often follows this procedure himself. He assumes that all who disagree with his version of liberal economics are " driven by an idiosyncratic abhorrence of the market economy and its political corollary " (p. 242), that all recent socialists base their hopes not " upon the power of their arguments but upon the resentment, envy and hatred of the masses " (p. 65), that Marxists " cling to historical materialism and stubbornly refuse to listen to its critics because they want socialism for emotional reasons " (p. 158), and that people who " support policies that aim at the substitution of planning by an authority for autonomous planning by each individual " are " longing for slavery " (p. 376). In a more restricted context he claims that " it is only the passionate pro-socialist zeal of mathematical pseudo-economists that transforms a purely analytical tool of logical economics into a utopian image of the good and most desirable state of affairs " (p. 367).

It is impossible to dismiss comments of this kind as disagreeable tricks of style. There are other comments which betray a tendency to sweeping generalisation as well as passionate feeling. " Marx misrepresented the operation of the capitalist system in every respect " (p. 117). " What prompted those who suggested the substitution of the social sciences for the sciences of human action was, of course, a definite political program "

(p. 259). " The religiousness of these sincere believers had nothing in common with the established system of devotion " (p. 339). Remarks of this kind suggest not liberal openness of mind but a refusal to leave familiar grooves of thought or, to use Professor von Mises' more technical language, too uncritical reliance on " thymological " explanations for shifts and diversities in idealogies. Whatever might be said about the differences between his approach to economics and the approach of economists who do not agree with him, it is clear that there would be an even bigger gulf between what he considers to be " thymological experience " and what other people would consider to be such.

Four points of more particular criticism raise important general issues. First, Professor von Mises, who disposes of many arguments, does not dispose of the doubts, insights and suggestions about value theory which made Gunnar Myrdal's recent collection of essays so illuminating. He writes too confidently about " ends " and " means " in his first few pages, and repeats the word " *wertfrei* " frequently in the remaining pages. It is neither malice nor evil intention which has made people doubt his version of value theory but genuine intellectual uneasiness. Second, as a historian Professor von Mises is less convincing than he is as a writer about history. He makes many extremely suggestive general remarks about history and the methods historians should use, but he grossly over-simplifies both the actual history of ideas and the history of institutions. It is not true to say, for instance, that " no proletarian or son of a proletarian contributed any substantial idea to the socialist idealogy " (p. 121) or that " workers were never enthusiastic about socialism." Interesting historical questions are side-tracked in this kind of dogmatic account of what happened in the past. Third, in his account of the operation of the economic system at the present time he does not examine carefully either legitimate discussions of " weaknesses " in capitalism or such interesting phenomena as rates of growth in socialist countries. Of the capitalists and entrepreneurs of a market economy, whose role has changed in different periods of economic history, he writes generally " they acquire and enlarge their property through the services they have rendered to the consumers, and they can retain it only by serving daily again in the best possible way " (p. 116). No attempt is made to distinguish between the theory of a market economy and the actual working of " free economies " in practice both in the past and at the present time. Fourth, he uses terms which provide props for his argument but never receive careful enough attention. The most important of these terms is " masses." He rightly accuses Marx of not having clearly defined what he meant by " class," but he tends to use the word " masses " himself as a very general term which some of his readers will query. There is a considerable truth in a comment by Mr. Raymond Williams in his *Culture and Society* that " there are in fact no masses; there are only ways of seeing people as masses." Indeed, Mr. Williams' book, which deals with the history of the use of

words in changing social contexts, brings out clearly why the kind of arguments advanced by Professor von Mises—many of them echo (often eloquently) old controversies—have not received the assent of a considerable number of intelligent people over the last hundred and fifty years.

It is a tribute, of course, to Professor von Mises that his trenchant exposition provokes vigorous dissent. How useful argument at this level really is must, however, be open to question. One reason why Professor von Mises' eloquence will not convince all his readers is not that they are stubbornly cocksure but, to use a phrase of Gunnar Myrdal, that they are " more modest " than he. They will doubt whether what the blurb of *Theory and History* calls the author's " frequent tilts at other experts " will generate more light than heat.

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The British Budgetary System. By SIR HERBERT BRITTAIN. (London: Allen & Unwin, 1959. Pp. 320. 25*s.*)

IT is hardly accidental that it is quarter of a century since the last edition of a comprehensive account of the British budgetary system appeared, bearing in mind the preoccupation of writers in public finance with the more fashionable and possibly more exciting study of the use of the budget as a stabiliser and redistributor of income. However, this very change in the policy uses of the budget has brought into being major alterations in the structure of the system of public finance, notably in the financial relations between central government, local government and nationalised industries. Sir Herbert Brittain's study is very welcome, all the more so because it is written with the inside knowledge of a former senior administrator of the Treasury.

There are two noteworthy features of this book. We would expect from Sir Herbert an authoritative account of the whole budget " cycle " from the preparation of estimates to the deliberations of the Public Accounts Committee. What is particularly useful in his survey is the documentation. This means that the book is more than a handbook for students and civil servants, but also a starting-point for detailed research into budgetary matters. The second feature is that the budget is explicitly regarded as an instrument of economic policy. It is obviously impossible to expect a book of this kind to preface description of our financial administration with a complete treatise on social accounting and the theory of fiscal policy. Sir Herbert has managed to summarise the rationale of fiscal policy in a few pages in Chapters II and III very well indeed, and as a result, the more sophisticated reader, at least, will discern more clearly the links between theory and policy.

One regrets that Sir Herbert has chosen to avoid controversy. Hilton

Young's book seemed to gain by its obvious bias and occasional stylistic flourishes; remember his nostalgic reference to Hume's and Gladstone's concern for public spending—" now in Elysium it is theirs bitterly to lament the wastage of Ambrosia." We must respect the author's views on this matter, but he invites criticism on two counts in this connection. First of all, having laid emphasis on the budget as an instrument of economic policy, he nevertheless deliberately avoids discussion of various important proposals for budgetary reform. Secondly, we are told in the Introduction that where controversial matters arise, the author has confined his comments to what he conceives to be the justification of existing practice, but a little later we are told that he does not purport to express " what may have been official views on the points under discussion." The reader may be excused if he has difficulty in identifying what is exactly the Treasury view. For example, is Sir Herbert's view on modern debt theory (pp. 53–4) or his discussion of the necessity for more information on the distribution of National Debt holdings (pp. 183–4) shared by to-day's Treasury officials? It would be interesting to know.

It is praise enough for a work of this kind that it makes the subject palatable. Sir Herbert Brittain's book does just this, but at the same time it preserves enough detail to make it an essential work of reference as well as a text-book.

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Studies in the Quantity Theory of Money. Edited by MILTON FRIEDMAN. (Chicago: University of Chicago Press (London: Cambridge University Press), 1957. Pp. 265. 37*s.* 6*d.*)

THE behaviour of monetary systems provides one of the most rewarding fields of empirical study to be found in the whole of economics, and this collection of four such studies, with an introductory essay by the editor, is to be warmly welcomed as a set of new explorations, or of extensions of previous explorations, in that field. At first sight the studies have little common character; Mr. Cagan's long article on the monetary dynamics of hyperinflation and Mr. Selden's on monetary velocity in the United States are attempts to establish econometric models explanatory of the courses of events: Mr. Klein's investigation of German money and prices from 1932 to the end of the War is more concerned with the establishment of the statistics required for calculating velocity, and with qualitative explanations of the changes in velocity: Mr. Lerner provides something more like a straightforward chronological account of monetary policy and inflation in the Confederacy. One common feature of these studies is, however, their use of the concept of velocity of circulation—a useful tool of analysis which has lately, for no very good reason, been out of fashion. Here it is introduced

along with the concept of demand for money with a commendable absence of inhibitions.

Mr. Cagan's model of the process of hyperinflation, which he tests with data from Germany, Poland, Austria, Hungary and Russia after the First World War and from Greece and Hungary during and after the Second, is somewhat similar to a less-formal one which the present reviewer used in an earlier work; it consists essentially of a relationship between the rate of increase of prices and the subsequent value of the velocity of circulation, or real value of the money stock. In the Cagan model the rate of increase of prices, which is taken as determining current expectations about the rate at which prices will continue to rise, is an average, with exponentially decreasing weights, of the rates of increase experienced right back to the start of the inflationary period. Having obtained generally close relationships between this and the real value of the money stock (which would, of course, be proportional to the inverse of velocity if real income remained constant), the author shows that unless prices are very sensitive to their past rates of change, hyperinflation will not be a continuously self-generating process without a continuous increase in the money supply. It seems that only in the German and Russian inflations was this sensitivity of prices to their past rate of increase sufficient for hyperinflation to continue indefinitely after a stabilisation of the supply of money.

This is an extremely interesting result. Mr. Cagan's attempts to go beyond it are, however, less convincing. His attribution of the increase in the supply of money to the desire of the authorities to obtain revenue by this means, and his denial that it is to any major extent due to pressure upon them to finance rising factor payments (rising because of expectation-inspired wage demands or the depreciation of the currency on the foreign exchanges) would be difficult to justify in regard to, at any rate, the German hyperinflation, and he presents no evidence that appears to justify it.

Space does not permit any comment on the very useful contributions of Mr. Klein and Mr. Lerner, except that it is a little surprising that even in Chicago the experience of the Second World War should not throw more light than the former writer, and the Editor, appear to have perceived upon the process by which really savage wartime controls depress the velocity of circulation. Mr. Selden's attempts to explain the course of velocity in the United States present, however, some very interesting features. Perhaps the chief of these, at first sight, is the discovery that the difference between long- and short-term interest rates is much better correlated with velocity than is either of these rates by itself, and that real income per head also seems to be a useful explanatory variable. The author's treatment of the excess of long over short rates as the " cost of holding money substitutes " (*i.e.*, short-term securities) constitutes an ingenious analogy with the Keynesian treatment of any interest-rate as the " cost of holding money."

To argue in this way is, however, to treat the velocity of circulation as if

it were determined by interest rates (or differences between them) acting as an independent variable. It is, perhaps, helpful to reflect that when money is scarce (velocity high) there is pressure to realise both short- and long-term securities, but more especially the former, because of their greater saleability and better maintenance of capital value; hence high velocity goes with relatively high short rates in relation to long rates. The interesting discovery that this difference is better correlated with velocity than is the *level* of rates, either short or long, could, nevertheless, hardly have been made if the author had, like many of his predecessors, been intent mainly on " explaining " the course of rates of interest.

The significance of real income per head in this connection is apparently greater for long-term trends than for short-term changes. It is, however, not by any means clear how far the relationships dealt with by Mr. Seldon subsist merely between simple trends in the variables. Indeed, it should perhaps be added that the statistical basis for the deductions in this study is open to some suspicion. The degree of inter-correlation between some of the variables which are treated as independent is not stated, but it may well be so high as to place the validity of the multi-variate analysis in doubt.

A. J. BROWN

University of Leeds.

Trade Balances during Business Cycles: U.S. and Britain since 1880. By ILSE MINTZ. (New York: National Bureau of Economic Research, 1959. Occasional Paper 67. Pp. x + 99. $1.50.)

THIS is an interesting and careful study of a subject which the events of the recent recession have made of topical interest. It is easy to make a list of the factors which in theory will determine the cyclical behaviour of a country's balance of trade: the relative magnitude and timing of cycles in it and abroad, relative price- and income-elasticities of demand, relative elasticities of supply and so on. These are complex and would permit almost any result. The empirical data, however, viewed over a long period, show a clear-cut pattern, which is all the more remarkable for having undergone, in the British case, a complete reversal after 1914. Before 1914 the British balance of trade rose in cyclical expansions and fell in cyclical contractions, whereas since 1914 the opposite has been the case. In the United States the normal rule has always been for the balance to fall in expansions and rise in contractions.

The American experience invites explanation in terms of a high income-elasticity of demand for imports. Dr. Mintz shows that this is only part of the truth and that a falling off in American exports towards the end of the expansion, followed by a recovery when the recession has begun, has been equally if not more important. The British experience since 1914 has been similar to the American in general result, but it has been customary to

explain the two rather differently, with less emphasis in the British case on the income-elasticity of demand for imports and more emphais on the terms of trade. Dr. Mintz attributes the reversal in the cyclical behaviour of the British balance after 1914 to the corresponding reversal in the cyclical behaviour of the terms of trade, already noted by Martin and Thackeray and others.

An important feature of Dr. Mintz's study is the technique of distinguishing " co-phases," in which the level of activity in the country concerned is moving in the same direction as that in the rest of the world, and " counter-phases," where the opposite applies. This distinction yields some interesting contrasts. Thus in Britain since 1914 the favourable movement of the balance of trade in contractions has been more pronounced in co-contractions than in counter-contractions, whereas in the United States the reverse has been the case. This confirms the notion that what mainly matters for Britain is the change in the terms of trade brought about by differing elasticities of supply of different classes of goods in face of an overall world fall in demand, whereas for the United States it is the *relative* levels of demand at home and abroad that are most important.

The phase technique does not apply quite so well to Britain before 1914. For this period a British boom is not something that can be thought of as an essentially domestic phenomenon and set in contrast to a world boom. It is shown that before 1914 Britain's balance of trade fell more in counter-contractions than it did in co-contractions, the reason being a less favourable movement in the terms of trade in counter-contractions. This is on the face of it a rather troublesome conclusion, since it appears to suggest that a world expansion was, after all, bad for Britain's terms of trade before 1914 as well as after. But inferences about the effects of world expansion drawn from occasions when world expansion coincided with British contraction are misleading, since such expansions were by their nature untypical. Britain's unique position as supplier of heavy industrial goods and as lender meant that as a rule a world boom meant an intense demand for British exports and a rise in their prices; a British co-expansion thus resulted and an improvement in Britain's balance and terms of trade. Occasionally, however, there were expansions abroad which, because they did not extend to our chief markets or for some other reason, did not raise the demand for our exports much but did raise the prices of our imports. These would presumably be the world expansions associated with British counter-contractions, but they would not give a representative picture of the normal effect on Britain of a world expansion. The loss of Britain's special position as a supplier of capital goods and capital after 1914 was no doubt an important reason for the reversed cyclical behaviour of the balance: a world boom no longer meant such a concentration of demand on British industry.

One interesting finding of the study is that cyclical movements in the balance in both countries were much stronger around the peak of the cycle

than at other phases. Significant non-linearities in supply functions would appear to be present. In Britain before 1914 this assumed a peculiar asymmetrical form: before the peak exports rose *more strongly* than imports; at or after the peak exports fell *sooner* than imports. Dr. Mintz regards this as a lag of imports after general business activity. Others have interpreted it as a lead of exports.

The paradoxical deterioration of the American balance of trade in the recession of 1958, which finds a parallel in the depression of the 1930s but in very few other cases, occurred too late to receive more than a postscript footnote from Dr. Mintz. Dr. Mintz is disposed to regard it as an abnormality rather than as a sign that changes in import structure or a tendency for American exports to become a marginal source of supply are causing her position to resemble that of Britain in the days of her world leadership.

Dr. Mintz's concluding speculations on the impact of balance-of-trade movements on the level of activity through their effects on foreign exchange reserves seem premature in the absence of an equally systematic study of other items in the balance of payments, especially capital movements. The latter are also given rather less consideration than they might seem to deserve in the explanation of the behaviour of the balance of trade itself, especially for pre-1914 Britain.

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Expectations, Uncertainty, and Business Behavior. Edited by MARY J. BOWMAN. (New York: Social Science Research Council, 1958. Pp. 202. $2.00.)

IT is probably generally agreed amongst economists that the theory of business decisions is in an unsatisfactory state. Indeed, a good case could be made for the view that the theory is held to be much more useless than it really is. In any event, this collection of papers and the conference which occasioned them are clearly inspired by the belief that a new and determined attempt should be made to understand the nature of business decisions. Moreover, it is held that such an attempt to be successful must involve a number of disciplines.

Almost all the papers here printed are interesting, some are extremely well argued, and the Conference was clearly worthwhile. On the other hand, it must be admitted that nothing that could be called a " new theory " emerges, that the psychologists speak a language which does not seem of immediate use to economists (and no doubt *vice versa*) and that when one has read them all one is not a great deal farther towards an acceptable method of analysis than one was before. However, since most issues ever raised are competently discussed in one or other of the papers, they should certainly be read by all who work in this field.

The book is divided into four parts: " Theories of the Nature and Structure of Expectations " (Georgescu-Roegen, Shackle and Ward Edwards); " Psychological Approaches to the Study of Expectations and Business Behavior " (Herbert Simon, Katona, Meredith, William E. Henry and Albert Lauterbach); " Effective Expectations and Time Horizons with Special Reference to Practices in Selected Industries " (Ruth Mack, Merton J. Peck and Hans Brems) and " Forecasting and the Role of Expectations in the Economy of the Firm " (Modigliani, Kalman J. Cohen, Robert Eisner, Irwin Friend and William Darcovich). It is clear that a comment cannot, in the space of this review, be offered on all papers.

Georgescu-Roegen's essay entitled " The Nature of Expectations and Uncertainty " combines a lucid and critical account of a number of probability and expectation theories with a very persuasive exposition of his contention that " the real number system does not suffice to represent all expectations and that those expectations which can be meaningfully connected with a real number form only a special class." This paper should be widely read in particular for its criticism of Savage's well-known book. To a non-specialist, in this field, Roegen's arguments are pretty convincing.

Professor Shackle's " Expectation and Liquidity " is a beautifully written account of his theory, together with an attempt—alas unsuccessful—to apply it to the demand for assets. The rather *ad hoc* machinery constructed for this purpose will not do. Indeed, I think one must recognise that the Shackleian apparatus, while it affords considerable psychological insights, is no more a complete theory of expectations and decisions than any other and is open to a number of rather important logical objections—some of which become particularly insistent when the demand for assets is considered.

Herbert Simon's paper " The Role of Expectations in an Adaptive or Behavioristic Model " is really excellent. Nothing that he says is startingly new (partly because he himself has already argued on similar lines on earlier occasions), but it is always to the point and very convincing. He draws the by now familiar distinction between " programmed " and " non-programmed " decisions, argues that the two types must be treated differently, shows that there are good grounds for believing behaviour to be " adaptive " rather than " optimising " and makes excellent use of the notion of " aspiration levels." Anyone on the way to firms armed with questionnaires will do well to read this paper first.

The psychologists' contributions cannot be readily assessed by an economist. However, the paper by Meredith, " The Surprise Function and the Epistemic Theory of Expectations," certainly holds the non-specialist reader's attention. He regards the individual in all his activities, as constructing models (expectations) of what the external world is like and presents (rather eliptically) a neuro-physiological account of this. From our point of view, the interest of this theory lies in the importance which it attaches to what Meredith calls " biography": that is, the kind of model of the

external world someone constructs will depend on the kind of man he is, and this in turn will depend on his past history. The reason why this view seems to me interesting is that it faces us again with a well-known problem: is it ever possible to construct a general theory of expectations and decisions, or must we recognise the overriding importance of the personal (biographical) element in these processes? One suspects that the answer would be that while biography is vital to our understanding of any particular decisions, social institutions and the like, assure that many individuals share similar biographies. One would like to hear the sociologists on this. (Lauterbach's paper is of some use here.) It may well be that a classification of businesses by "biographical" types would be more illuminating for some purposes than the usual "industry." Mr. Meredith seems to me unduly optimistic about the willingness of businessmen to allow economists or others to sit on important boards for research purposes.

The empirical papers are all interesting and well worth reading. Both Ruth Mack and Merton J. Peck suggest that uncertainty (in the Knightian sense) is probably less important in inventory decisions than has hitherto been supposed, and they examine a number of ways in which the businesses they have studied organise their information concerning the future. Hans Brems constructs a model on the basis of which he concludes "that the timing of expected rival response to price changes is radically different from the timing of expected rival response to changes in the non-price variables . . . (and that) in such circumstances, profit maximisation will turn the odds overwhelmingly against price competition." He illustrates his contention with reference to the automobile industry. Robert Eisner uses known plans (*ex ante* data) and actual sales to test a sensible and fairly traditional theory of investment decisions. His conclusions are tentative, but nonetheless very interesting, particularly since they are not in noticeable conflict with current theory. Modigliani and Cohen provide a theoretical discussion of "The Significance and Uses of *Ex Ante* Data." This is an important topic at a time when we can look forward to an increasing flow of such data.

All in all, a most interesting collection, of which a review can only give the most imperfect of accounts.

F. H. HAHN

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Micro-economic Theory: A Mathematical Approach. By J. M. HENDERSON and R. E. QUANDT. (New York and London: McGraw-Hill, 1958. Pp. xii + 291. 58*s*.)

THIS is a pleasant, well and succinctly written text-book suitable for beginners in mathematical economics. Besides an introduction, there are seven chapters and an appendix on mathematical techniques. These are

elementary determinants, calculus and difference equations. The chapters are entitled: " The Theory of Consumer Behaviour," " The Theory of the Firm," " Market Equilibrium," " Multi-market Equilibrium," " Monopolistic Competition," " Welfare Economics " and " Optimisation over Time." It is seen that the book covers a wider field than its title would lead one to believe. Indeed, a major criticism of an otherwise laudable volume is that far too much ground is covered in a superficial way and too little in depth.

The superficiality is of two kinds: lack of precision at some important points of the argument and a failure to follow on discussion to its proper conclusions. A few examples of the first kind are:

(i) The student is given the impression that utility maximisation assures that indifference curves are everywhere convex (p. 14), while, of course, they only assure convexity at the chosen basket. The same mistaken impression is given with regard to isoquants (p. 51).

(ii) The impression is given that utility maximisation implies *strict* convexity of indifference curves, and hence single valued demand functions (p. 21). This is not so.

(iii) It is argued that the concept of consumers' surplus requires cardinal utility measures (p. 22 footnote), which is not true. (The author gives only the Marshallian definition.)

(iv) The symmetry of the off-diagonal elements in the well-known Hicksian bordered determinant is used in discussion but neither proved nor commented upon.

(v) Houthakker did not prove that, given his axioms, the consumer must possess an indifference map (p. 33). The frontier of all points not inferior to some collection x and all points not superior to that point is not necessarily a collection of points between which the consumer is indifferent.

(vi) The term " gross substitutes " is used (p. 123) and not defined anywhere.

(vii) The integration of money into a general equilibrium analysis is carried through faultlessly, but the whole section is quite spoiled by the nonsensical conclusion that the quantity of money *determines* the level of money prices *because* it can be shown that equilibrium money prices vary directly with the stocks of money (p. 145). In the model the authors use the equilibrium rate of exchange of any good for money cannot be determined from a knowledge of the stock of money alone. Nor could an index of such exchange rates. It is simply a wrong and misleading use of the word " determine."

(viii) The marginal rate of substitution between any two items consumed in any one " week " must in equilibrium equal the relative prices of these two items ruling in that week. Hence this equilibrium

marginal rate of substitution, in a multi-week model, is independent of interest rates. From this, the authors conclude that the utility function $U(x_1 \ldots x_T)$, (where x_i is a vector of items consumed in week i) may be written as $V(c_1 \ldots c_T)$ (where c_i is total consumption outlay in week i). This, however, is only true if the marginal rates of substitution between any two items in any one week are independent of the amount consumed of any item in any other week (separability).

These matters are perhaps not too serious. Indeed, there are few books in this field where a similar list could not be made. But the failure to pursue any one subject to its proper conclusion and depth is a different matter. Examples of these are:

(i) There is no discussion of consumers' surplus (except the footnote already mentioned), and no discussion of index numbers. Nothing on the Leontief–Hicks theorem on aggregation. Nothing on the Le Chatelier principle (which the mathematics used could easily have dealt with). Nothing on the application of programming to utility analysis (although programming is discussed elsewhere). Nothing on liquidity preference and the demand for assets, although the Von Neumann index is described. Nothing on insurance and gambling. Nothing on bilateral exchange.

(ii) The discussion of production makes no mention of the elasticity of substitution and the uses to which it has been put. There is no mention of innovations or their possible classification. Separable production functions and their uses in defining "factors of production" are absent from the chapter.

(iii) The chapter on market equilibrium makes no use, and contains no discussion of, the correspondence principle, although both stability and the effects of taxation are discussed separately.

(iv) No connection is established between the discussion of community indifference curves, etc., in the chapter on Welfare Economics and the uniqueness of equilibrium analysis of an earlier chapter. There is nothing on the role of "income effects" either in stability or in welfare economics. Too little is said on the economics of "free goods." There is no "comparative statics" analysis of the multi-market model, although the mathematics used would have been perfectly adequate for this.

(v) Only one kind of monopolistic discrimination is discussed. There is nothing on advertising or product differentiation. Although six theories of duopolistic behaviour are given, they are neither evaluated, criticised nor compared.

(vi) There is no discussion of the valuation of capital goods and intermediate products. Nothing on the definition of capital. A snippet on the period of production. No mention of some of the more important recent theories of savings.

Clearly there is much to be said for a short book, and if the authors had done all or some of the things mentioned they would have had to leave out others. But so they should have done. Micro-economics is really quite large enough a topic without discussing welfare economics and general equilibrium analysis as well. Instead, a chapter on decisions under uncertainty, one on savings and one quite separate one on production and programming would have been made better fare.

When all this has been said, one must hasten to add that, in spite of all this, the book is an impressive achievement. The presentation is everywhere clear and concise, and the authors are thoroughly at home with their subject. The teacher, provided he is willing to supplement the gaps in lectures, can certainly recommend the book to his students.

F. H. HAHN

University of Birmingham.

The Challenge of Development. (Jerusalem: The Eliezer Kaplan School of Economics and Social Sciences, the Hebrew University, 1958. Pp. 233. 7*s*.)

THIS symposium, held in Jerusalem in 1957, includes contributions to the sociological, political and demographic as well as economic aspects of development. Much the most provocative and stimulating of the specifically economic papers is Professor Kahn's "The Pace of Development." Concentrating on societies in which the labour force is far from being fully utilised, he makes a strong case for the adoption of a bold strategy in making the steep ascent from backwardness, justly emphasising that "a high rate of development from the outset involves current benefit as much as current hardship, quite apart from all-round benefit which gradually results from the improvement in the means of production." There is a salutary plea for exploiting traditional methods of construction on a greater scale (compare the Chinese irrigation record in the winter of 1957–58) and an equally salutary warning of the use of global investment coefficients.

Given an abundant supply of unused or only partially or "imperfectly" used labour, Professor Kahn is surely right in inverting the conventional formulation by claiming that it is "as true, and perhaps more illuminating, to say that the proportion of the current income of the community devoted to capital formation depends in the rate of economic development." Much of his essay is devoted to the optimum methods of securing a greater redistribution of consumption in order to facilitate a higher volume of employment. It must be added that recent experience serves to confirm Professor Kahn's case for the setting of ambitious targets and for fuller utilisation of the labour reservoir, which if properly tapped constitutes perhaps the greatest single asset of a large under-developed country bent on rapid economic growth.

Other contributions include papers by Professor Kuznets on " Economic Growth of Small Nations " and by Professor Patinkin on " Secular Price Movements and Economic Development." In his essay on " Monetary Policy in Underdeveloped Countries," David Horowitz, the Governor of the Bank of Israel, reviews the dilemmas of a central bank in combating inflation when state enterprise is substituted for private initiative and " inflationary saving " for the formation or influx of capital. Professor Bachi's " Trends of Population and Labour Force in Israel " provides the basic demographic background to Israel's economic problems.

S. ADLER

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Business Budgets and Accounts. By H. C. EDEY (London: Hutchinson, 1959. Pp. 174. 18*s.*)

THIS book, by providing an introduction *appropriate to economists* to the principles and practices of business accountancy, makes good a serious deficiency in economic literature. For though we economists have by now become quite sophisticated about the preparation of statistical statements, of both the *ex post* and the *ex ante* variety, relating to an industry or an economy, we have remained very ignorant about the equivalent procedures within the individual firm. We can find our way through the complexities of the Blue Book on *National Income and Expenditure* but are helpless when faced with a corporate profit and loss account; we analyse with confidence a consolidated balance sheet of " the Banking Sector " but stare impotently at the balance sheet of the Bowater Paper Corporation; we are at home with the *Economic Survey* and have a contemptuous familiarity with the antiquated budgetary procedures of H.M. Government, while being scarcely aware of the existence of business budgets; we have learnt from Professor Devons and Mr. Nove about production programmes in the M.A.P. and the U.S.S.R. but not about " the physical production plans that decide," within an individual firm, " what work is to be done, and in what manner." Such, at any rate, is the ill-balanced state of the English economist's competence: I am not sure that our colleagues in America and on the Continent would be so uniformly open to the same criticism.

Now, however, we have Mr. Edey to guide us into the unfamiliar territory. His book is, of course, only an elementary introduction, but this is perhaps what we most need: a sympathetic lead into the existing specialist literature. For the accounting profession has its own tasks to perform, its own concepts, its own way of looking at things, its own convenient fictions, its own mumbo jumbo; and all are different from the economist's. In this foreign land, where fact and fiction seem to be inextricably intermingled, we need at first the help and advice of a fellow economist who already knows the local language and customs, and this is precisely what Mr. Edey offers us.

He explains in his introduction that the business accountant has to satisfy different masters: the shareholders, the directors, the tax authorities and (in case of liquidation) the courts. Rightly, in this book, he concentrates on the role of accountants in assisting the directors: this they do (he explains) by facilitating the setting and the achievement of three " targets ": a target for revenues and costs, a target for the availability of money and a " target pattern of resources other than money " (p. 12).

These targets are commonly recorded in three principal budgets, each further subdivided and each covering every month in the forthcoming year: the general profit and loss budget, the finance budget (recording the planned flows of money into and out of the business) and the budgeted balance sheet. In a passage which might equally serve as an apologia for the *Economic Survey* or a war-time aircraft programme, Mr. Edey explains that—

> " the preparation of the budget imposes a discipline on all members of the organization, in that it causes them to consider and reflect on what they intend to do. Furthermore, the budgeting procedure, if properly carried out, provides a safeguard against different departments of the business having mutually contradictory plans. And once the budgets have been established and approved they provide an operational plan of action for the guidance of all concerned. The checking of the actual results (the figures for which are provided by the accounting system) against the budgeted figures provides information on the course affairs are taking, and this may lead to changes in the way things are being done, or in the plans and budgets themselves, or in both. The comparison may also help to improve budgeting procedure in later periods."

" The year seems to be a natural period for human planning and most businesses that have budgets prepare them annually." This surely means that the figures relating to the later months of the forthcoming year will, when this time arrives, already be months out of date as forecasts and perhaps quite inappropriate even as targets. One is therefore surprised to learn that relatively few businesses adopt " a continuous process of annual budgeting at . . . monthly or quarterly intervals ": nor (even more surprisingly) does Mr. Edey believe that they should: " There is something to be said for getting a definite plan for a definite period into one's mind and keeping it there until the end of the period, even though you know that adjustments will have to be made to it as time runs on " (p. 46).

The passage I have just quoted is the only case where I found Mr. Edey's views less than completely persuasive: otherwise he manages not only to anticipate all the queries which occur to an economist-reader but also to provide answers which struck me as well balanced and reasonable. I therefore commend his book " not merely to newcomers to economics but also to old hands seeking an introduction to Business Budgets and Accounts."

BRIAN TEW

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Introduction to Operations Research. By C. W. CHURCHMAN, R. L. ACKOFF and E. L. ARNOFF. (New York: John Wiley (London: Chapman & Hall), 1957. Pp. 645. 96*s*.)

ECONOMISTS have become accustomed to the rate of growth in terminology exceeding that of the subject-matter with which it purports to deal. Small wonder, then, that one approaches a book on " Operations Research " with some misgivings. But reassurance comes quickly, for these two words singly or in combination stand not for yet another theoretical variation but for an attempt to apply existing techniques, suitably modified, to the practical solution of business, military and government problems. As befits such inquiries, the approach is eclectic. More significantly the emphasis is on research by teams composed of a number of specialists. The potential gains from specialisation are applied to the discipline of research itself.

The training grounds of operations researchers would appear to be mathematics, engineering, natural science, psychology and economics, together with the administrative units to which the study refers. If this book is representative of the field of operations research, there is a clear indication that mathematics has an important part to play.

The feature common to all the problems is that of adapting available means in the best manner to attain the specific ends of the interest group concerned. An economist might therefore be surprised that there is no discussion in terms of utility, preference systems, opportunity cost and the like. But the three authors of this volume do not include an economist within their number though one is well aware ot much economic sense and judgment. This is particularly apparent in the chapter on " Weighting Objectives," which overlaps the problems economists have been grappling with in welfare economics.

The writers recognise that these objectives are largely non-economic and often non-quantitative and go on to suggest that since, for useful solutions, some weights must be attached this is best done clearly and specifically. But at this point the argument becomes blurred. Methods for the derivation of weights are advanced which could lead the less-discerning reader to conclude that these weights are a good deal more objectively based than in fact they are.

There is also too great a confidence in the ability to quantify such aims as " retention of family control in a business." The problem is correctly framed, but there is too much impatience to produce answers. Fortunately in practice the problems with which operations researchers have been most successful are those where it has proved possible to minimise or eliminate the problem of weighting. Attention is focused on the choice of optimum methods of organisation within firms rather than within society or industry.

Analysis accompanied by practical illustration is provided for five

different classes of problem: inventory models and the choice of optimal inventory policy; linear programming and allocation policy; queueing models, an aspect of the theory of stochastic processes, and their application to traffic and pipeline congestion; replacement in theory and practice; games theory and its relation to the explanation of competitive bidding. In each case the treatment is well graduated and the argument lucid, though the non-mathematician will rather quickly find the going heavy. This is no bad thing. The tools used and the clarity and relevance of the many practical examples are such that some investment in the necessary mathematics will prove worthwhile.

Of more immediate concern are the conclusions that may be drawn by the more mathematically minded. To illustrate, in the chapter on replacement models the correct timing of replacement is analysed in terms of the minimisation of cost under conditions of known life and effectiveness, on the one hand, and on the other, in terms of the probability distribution of the effective lives of machines. At no point is any attention given to expectations of demand, which may have marked effects on the choice between machines with different lifetimes. Hence, it is not surprising to find the name of F. A. Lutz does not figure in the extensive bibliography.

What, then, does this add up to?

This is an important book, full of fruitful ideas, and useful techniques with a growing field for application. But two things are clear; the economic student must be well-versed in his discipline before digesting the contents, and the operations research team on any project should most certainly contain a well-trained economist.

MALCOLM R. FISHER

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The Motor Industry. By G. MAXCY and A. SILBERSTON. (London: Allen & Unwin, 1959. Pp. 245. 25*s*.)

THIS is a book of much wider interest than its title indicates. It is not only that everyone can claim some familiarity with the industry and its products so that it provides acceptable illustrations for text-books. The motor industry is, in a sense, the archetypal industry of the early twentieth century in much the same way that the railway mirrored the forces of industrial development in the mid-nineteenth century and cotton those in the late eighteenth. The motor car is the typical durable consumer-good and is of special interest, therefore, in demand analysis. It is produced by techniques extending from the craftsman and the small workshop to the automatic factory, and illustrates both the progress of technique and the influence on it of scale of production. Since technical change is rapid and producers are few in number, it also provides a useful case-study in oligopoly.

The demand for motor cars is already the subject of a considerable

literature, but there is a lack of studies concentrating on the circumstances governing supply. The authors furnish a very readable account of the structure of the industry and of the workings of competition in it. But they do much more: they attempt to give quantitative expression to such concepts as the technical optimum and the short- and long-run cost curves of the firm. Both curves, although downward sloping, show too gradual a fall in relation to the presumed elasticity of demand to make price-competition a worth-while gambit. In the short run, prices tend to be kept stable or to be adjusted in line with the variable cost of labour and materials; in the long run, competition takes the form of trying to hit the public taste more successfully with a new model at the going price.

The authors suggest that, in the short run, variations in output have relatively little effect on unit costs; for a weighted average of cars, vans and trucks, fluctuations between 20% above and 40% below "capacity" are unlikely to cause standard costs to vary by more than 5% either way, although for a single model the curve may show a slightly steeper slope. This is taken at other stages in the argument to show that fixed costs are not particularly heavy; they come to only 16%, at standard volume, for the firm supplying the particulars, and to about 13% at full capacity. It is, however, a little misleading to quote ratios of this kind, since the variable costs of the car manufacturer are equally low, and nearly 70% of total factory cost in the example given represents bought-out parts and components. Suppliers have their own fixed costs; but do they quote lower prices when demand falls off? The authors hint, when discussing the advantages of vertical disintegration, that they may do so; they do not raise the issue in relation to the short-period cost curve.

The long-run cost curve seems to fall more steeply than the short at outputs under 100,000 a year and less steeply at outputs above 200,000. The authors argue that there are technical economies in body pressings (and perhaps some components) up to outputs of 1 million a year, in machining up to the half million mark and in assembly up to about 100,000. They give figures, supplied by one of the larger firms, that imply a fall in unit costs of only 13% through a fourfold expansion from 100,000 to 400,000 a year. But this estimate is certainly too conservative: for example, it shows no fall in fixed overheads after 300,000, no reduction in variable indirect costs (supervision, maintenance, etc.), and takes no credit for economies accruing to suppliers as a result of the expansion. It is not altogether clear to the reader whether the curve is supposed to relate to an integrated firm making its own bodies, an ambiguity of no little importance when the cost of the body is one-third of the factory cost of the car and the major economies of scale at volumes in excess of 100,000 are in body-building. If the curve does relate to an integrated firm it is difficult to fathom what is included in the 70% of total cost that goes on components and materials; but if it is not, then the curve seriously underestimates the technical economies of scale.

The authors themselves suggest that doubling output to 200,000 units would achieve a saving of 10%, and a further doubling to 400,000 would increase this to 15%: if the output of the *industry* were expanded fourfold they would expect a reduction in costs of one-fifth to one-quarter. It is not altogether easy to reconcile these estimates with the fall in real costs per vehicle between 1930 and 1937, which they put at 35% (for a doubling of output), or the subsequent fall of perhaps 20–25% between 1950 and 1955 (when output increased by 50%). No doubt, as the authors emphasise, the average size of car was declining in both periods and there was also some progress in the technique of manufacture independently of the effects of expansion. Nevertheless, the reader may feel that on one of the major issues the authors are under-stating their case. He may also feel that they leave him with a very vague idea of the proportion of British output consisting of models in the 100,000-a-year class, and no idea at all that the scale on which the Volkswagen is manufactured is 3,000 a day.

There are a number of other questions to which one would like a more definite answer. If a firm produces a successful model, why does this not give it a cumulative advantage, even at moderate demand elasticities? Why do some models succeed while others fail? Why do the Volkswagen and Dauphine occupy a place in their own countries that no similar car occupies in Britain? If variety is costly, what elements of variety cost most, and are they the elements that most enhance the appeal of the car? It is possible to assemble 1,000 cars a day, each different from the others, at no appreciable extra cost, provided they use the same jigs, spraying sheds and so on; but it is not possible to give them all a different silhouette. Could standardisation be carried further without injury to sales? Or are manufacturers the prisoners of their own marketing organisation, forced to imitate their rivals and yet offer something recognisably different? At what rate is the technical optimum expanding from one decade to another, and is the lag of British firms behind the optimum growing or contracting?

If these questions are not answered it is not because they have escaped the authors. They have done a workmanlike job and give to the non-technical reader a vivid picture of the things that matter to those who manage the industry.

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Effects of Mergers: Six Studies. By P. Lesley Cook with the collaboration of Ruth Cohen. (London: Allen & Unwin, 1958. Pp. 458. 42*s*.)

Most studies of industrial combination are concerned with a description of the processes of change and of the forces that give rise to them. The authors of this book, while providing a useful account of the organisation of six industries over a long period, also try to estimate what might have happened in the absence of the mergers they describe and, on the basis of

this judgment, to reach a conclusion about whether the public interest would have been better served by any likely alternatives. Investigators of this type of problem usually limit themselves to a judgment of whether, *at a particular point of time*, the organisation and behaviour of an industry are open to objection on the grounds of public interest. They may extend their judgment beyond the present and speculate about the future effects of the monopolistic organisation under review. In doing so they may have to take account of operations in the recent past. But they are not usually called upon to consider how a situation, which is the result of a chain of decisions in the past, might have differed if the initial decisions had not been taken. Indeed, since such decisions are irrevocable, the alternative situations that might have arisen are irrelevant to any criticism of the present organisation. However, the task which the present authors have set themselves is much more ambitious than that of inquirers into contemporary industrial organisation, for they have sought to reconstruct a series of historical situations and to judge successive decisions over a long period of time. This approach brings a lively interest to their study, even though doubts may sometimes be entertained about the validity of particular reconstructions.

In the case of the cement industry, the authors conclude that, in the absence of mergers, concentration in relatively few firms would ultimately have occurred by the expansion of the more efficient, but that the elimination of other firms would have been long delayed because of market imperfections. This would probably have kept costs higher than they actually were, and prices would have fluctuated more widely. Profits would have been lower if competitive conditions had prevailed, but in all probability restrictive cartels would have been established from time to time with unfavourable effects on both costs and prices. On balance, it seems unlikely that any alternative course of development to that actually followed would have yielded substantial advantages from the standpoint of the public interest.

For calico printing the same conclusion is reached, though with some hesitation. Again, the authors are impressed by the fact that competition (in the absence of the combine) would have been too imperfect to eliminate quickly the less efficient firms. They seem to give insufficient weight to the industry's arrangements for curbing price competition and sharing output, arrangements which might have been less effective in the absence of the C.P.A. The authors' conclusions about the soap industry, where " bigness benefited the consumer," are also debatable and do not emerge clearly from their description of the course of events. It is shown that for many years the Lever combine failed to take advantage of its power to rationalise production and sales, and that selling costs, in consequence, were unnecessarily high. They refer to the merits of size in connection with research and development, but they fail to observe that many innovations in soap-making came from the Italian industry, which is composed of a large number of competitive firms. In the case of the glass and motor industries, mergers

played a minor part in the evolution of a highly concentrated structure, and the discussion, therefore, resolves itself largely into an examination of the merits of the economies of scale in those trades.

In general, the authors find that the disadvantages of mergers lie in the short-run effects on prices, and that these are offset by the elimination of inefficient firms and capacity, by the reduction of the waste that attends imperfect competition and by the technical and commercial economies which large firms can promote. Mergers, they say, are unlikely to be successful unless there are considerable economies of scale, and they are unlikely to take place unless these are difficult to achieve without mergers. In other words, mergers can be regarded as a method of bringing about a " higher organisation " in an industry (to use Professor D. H. McGregor's term), and in the industries under review the authors think that the mergers have achieved this result. Their conclusion, however, does not mean that competitive forces have been inconsiderable factors in promoting progress, for " long-period price competition " has seldom been " stifled to any important degree." It may be thought that the survival of competitive pressures in these industries was a condition of their continued economic efficiency, and that if attention were to be concentrated on the short periods in which the industries were able to escape from these pressures the verdict would be less favourable. This is borne out by the authors' criticisms of the mergers at certain stages in their growth. It is not to be expected that outside criticism can often strike at the foundations of a long-established industrial organisation. Such criticism can be most usefully applied to an industry at a moment when its organisation is in process of change and to particular aspects of its behaviour. Probably the chief value of the book lies in the chapters where these detailed criticisms are proffered rather than in the final verdicts.

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Malaya: A Political and Economic Appraisal. By L. A. MILLS. (Minneapolis, Minn.: University of Minnesota Press (London: Oxford University Press), 1958. Pp. xi + 234. 30*s.*)

THE author of this book, who is Professor of Political Science in the University of Minnesota, is already well known for his writings on South-East Asia; this addition to them is to be welcomed.

Professor Mills' first five chapters are mainly in narrative form; and the story that they tell is primarily that of recent political and constitutional developments in Malaya and Singapore, but at intervals there are comments on economic topics. Many of these are interesting and refreshing: Professor Mills is, for example, more critical than most authors of the role of the rubber companies and of the planters during the 1920s. His economic

paragraphs briefly sum up the story of the growth in Malaya of commercial agriculture and of a rich entrepot trade, and show the effects of these developments on a Malay population who had traditionally been subsistence rice farmers and fishermen.

The first of these five narrative chapters takes the story up to the outbreak of the war with Japan. The second is concerned with the " change and reconstruction " following the Japanese surrender: here Professor Mills handles critically, but fairly, the astonishing episode of the MacMichael mission and its attempt to combine all the Malay States into a Malayan Union; and he rightly points out that it was this episode more than anything else that " wrought the miracle of making the Malays lose their indifference to politics." Chapter 3 is entitled " The Communist Rebellion "; it starts with an account of the first, infiltrating phase of Communism in post-war Malaya, and passes on to the emergency of 1948, to the Briggs Plan, and to the Templer régime. In the case of the last two topics there is a tendency to stick a little too closely to narrative; Professor Mills does not allow himself to be enticed sufficiently far into comment and criticism, and his treatment of the " New Villages " hardly lives up to the promise in the publisher's blurb that " it may well be controversial." He is, however, interestingly critical about the virtual suppression of the Secretariat for Chinese Affairs, an action which is said to have deprived the Government at a vital moment of invaluable links with Chinese opinion, feeling and sources of information. Chapters 4 and 5, on " Communalism and Self-government " and " *Merdeka* " respectively, deal with moves towards political independence in post-war Malaya against the background of a plural society in which political parties tend to be communal in character; it takes the story up to the formation of the Malay–Chinese Alliance party and the coming of independence to the Federation of Malaya on August 31, 1957.

Chapter 6 departs a little more from strict narrative form than its predecessors, and considers the special economic, social, political and strategic problems of Singapore. Chapter 7 follows naturally, since it is concerned with Malaya and Singapore in Commonwealth defence.

Professor Mills concludes with two chapters on economic affairs, which give a very useful and readable summary of the present state of the rubber-planting industry in relation to such considerations as the probable future demand for natural rubber, the tendency for capital to be discouraged by the political climate, and the taxation structure in Malaya. The discussion of other facets of the Malayan economy is rather less adequate, and there is hardly any mention of the problems of rice production and peasant farming (apart from rubber small holding).

Professor Mills' book is a useful summary of the recent political and constitutional change in Malaya and Singapore, though it stops just short of very recent events in the latter. His account is fair, readable and generally

accurate. But it is not in the nature of this book to be profound or analytical, or to discuss such general issues as the suitability of representative government in a plural society or the impact of social change on politics. His economic comments are often shrewd and interesting, and his book can be read with profit by anyone seeking an introduction to Malayan economic affairs. But it is hardly a book for the expert on Malayan economics or on the economics of an under-developed society.

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Land Reform in Japan. By R. P. DORE. (London: Oxford University Press, 1959. Pp. xvii + 510. 55*s*.)

Essays on Japanese Economy. By SHIGETO TSURU. (Tokyo: Kinokuniya Bookstore, for the Institute of Economic Research, Hitotsubashi University, 1958. Pp. 24.)

THE dispossession of landlords, especially absentee landlords, and the transformation of tenants into peasant proprietors are regarded by many people as the solution to rural problems and the key to greater agricultural output. There may be excellent social and political reasons for land reform of this type. It is much more doubtful whether it will lead to greater efficiency and increased productivity. Japanese experience provides a useful testing ground for the student of land reform. Before the War Japan had a mixed system of tenants and peasant-owners under which considerable agricultural progress was made. But there was increasing dissatisfaction among the tenants in the inter-war period that they were receiving less than their due share of the national bowl of rice. Measures of rural relief by a sympathetic government in the 1930s reduced peasant discontent but left unsolved the basic tenancy problem. The post-war occupation authorities, with the support of many Japanese, implemented their policy of " democratisation," not only by dispersing the *Zaibatsu*, dedeifying the Emperor and encouraging the trade unions, but also by carrying out a widespread reform of the land-tenure system. The background, implementation and effects of this revolutionary institutional measure is the theme of Professor Dore's scholarly study.

The author has used Japanese written sources and, equally important, personal observation in the villages, to give us an exhaustive and authoritative account of this subject. Although his sympathies generally lie with the tenants, he discusses with balance and insight such emotive topics as the contribution of landlords to agricultural development. Among them were enough " energetic and keen agriculturalists . . . to contribute a great deal to the increases in productivity which took place in Japan after the Meiji restoration." He makes clear that the system of land tenure was only one

among many factors responsible for rural distress and is equally careful to avoid dogmatic statements on the causal relations between rural distress and military aggression. Much of the book is devoted to the social and political structure of the villages. His detailed case studies of households and hamlets at times interrupt the flow of the narrative, but they enliven the text and provide a salutary reminder of the dangers of generalising about Japanese rural life.

The changes in land tenure have been hailed as the most successful reform in Japan since the War. One can justly criticise many writers, including Professor Dore, for not stating rigorously the criteria by which this success is gauged. The impression I get is that a politically quiescent peasantry was one of the major reasons behind, and one of the main beneficial effects of, the reform. The economist will judge its success in terms of, *inter alia*, utilisation of resources, productivity and the contribution of agriculture to general economic development. That there have been remarkable improvements in agriculture and in rural living standards in recent years is clear. To a large extent these are to be attributed to government support and abnormal post-war conditions rather than to changes in tenure. The reform has not removed the basic problem of scarce land and capital relative to labour. In fact, it has reduced the average size of holding. In his chapter entitled " Permanent Reform or Temporary Interlude " the author also discusses the difficulties of preventing land alienation and the revival of tenancy in a future period of depression.

Professor Dore's book must certainly rank as one of the most significant contributions in English to Japanese economic history. It is to be hoped that in a few years time, when the new system has been operating longer and when the effects of cheaper food imports and reduced government aid may be felt, the author will give us a further assessment of the role of land reform in the Japanese economy.

Professor Tsuru is one of Japan's best-known American-trained economists. This volume is a collection of eight articles previously published in Western and Japanese journals and, like the curate's egg, it is good in parts. Part I contains six essays on post-war problems of the Japanese economy. Although these were important and topical at the time of writing, many of them are now dated. The most valuable is the article on " Employment in Japan: Problems and Prospects," which focuses attention on the difficulties of absorbing the rapidly growing labour force and stresses the need to increase exports if full employment is to be achieved and maintained. There is also a useful study of macro-economic planning in Japan, illustrated with many statistical tables. In Part II there is a long essay, written in 1941, on " Japan's Economy under the Strain of the China Incident." This was admittedly penned without adequate statistical data. It illustrates the dangers of making economic prophecies, for the author believed that by 1941 the China war had almost exhausted the Japanese

economy. The second historical essay on "Economic Fluctuations in Japan 1868–1893," reprinted from the *Review of Economic Statistics*, Vol. 23, No. 4, is the best known and most important in the book. It is a concise, well-illustrated analysis of the cycles which occurred during the vital formative period of Japanese economic growth.

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Furness and the Industrial Revolution: An Economic History of Furness (1711–1900) and the Town of Barrow (1757–1897). By J. D. MARSHALL. (Barrow-in Furness: Barrow-in-Furness Library and Museum Committee, 1958. Pp. xxii + 438. 23*s*.)

FOR much of the period covered by this study, Furness lay outside the main stream of industrial development, although its coastal position and the early development of the charcoal-iron industry meant that it was subject to a variety of external influences. In the mid-nineteenth century, however, the coming of the railway and the establishment of the Hindpool iron works transformed the area. For twenty-five years the history of Furness, and more particularly the town of Barrow, is an admirable example of break-neck industrialisation. Then, in the last quarter of the century, the growing use of non-phosphoric and imported ores, and the subsequent exhaustion of the hæmatite mines, brought economic difficulties from which the area was rescued only by the development of the naval shipbuilding yards at the very end of the century.

There is much in this volume to interest the student of railway history and of the general industrial history of the nineteenth century: Dr. Marshall tells his story with a wealth of detail and makes full use of the available statistical material, and he certainly casts his net unusually wide. For example, he traces the consolidation of landownership in the eighteenth and early nineteenth centuries and the increasing social stratification which accompanied it—a topic of peculiar interest in an area which at the beginning of the eighteenth century could still boast a predominantly peasant society. Nor does he neglect the human aspects of his subject: considerable attention is devoted both to the part played by individuals in the development of the district—though the author is certainly more critical of the activities of the great industrialists than is at present fashionable—and to the effect of social change on the life of the people.

On the whole, Dr. Marshall takes a decidedly pessimistic view of the human consequences of industrialisation, and this reviewer, at least, finds much of his argument compelling. It must be confessed, however, that his account rests heavily on evidence connected with housing and educational facilities,

general social conditions and mortality rates, and rather less is said about purely economic desiderata. The local materials for a study of wages and prices are admittedly scanty, but what Dr. Marshall has to say on this score is not entirely above criticism. He points out, for example, that the deflation of local wage-rates by Silberling's price index gives a spurious impression of rising real wages after the Napoleonic Wars, and I am not altogether clear, therefore, why the fact that wages lagged behind the index during the war period should apparently be accepted at its face value, despite the evidence of increasing expenditure on the poor. (Incidentally, the author's comparison of wages in eighteenth-century Furness with those in other districts is somewhat misleading: the reference on p. 40 to the movement of wages in the " country at large " should be to the *county* (of Lancashire) at large, and the error is repeated in a quotation from Mrs. Gilboy's study of eighteenth-century wages in a footnote on the same page.)

Some of the most interesting, and perhaps also the most controversial, pages of this study are those devoted to demographic questions. Dr. Marshall thinks that the population upsurge in the late eighteenth century should be mainly attributed to a steadily depressed death-rate. It is difficult to demonstrate the point statistically, and the figures he quotes in fact suggest that the birth-rate rose and the death-rate fell by roughly the same amounts. This is not surprising, since the population estimates on which the rates are based were apparently derived from the figures of baptisms and burials on the assumption that changes in birth- and death-rates tended to cancel each other out. In so far as there was a decline in mortality, however, he evidently considers that it took place in all age-groups, and not primarily among juveniles, because burials of " sons " and " daughters " constituted roughly a third of the total number of burials recorded in the registers throughout the century. But he has surely overlooked the probable effect of increasing numbers on the age structure of the population. During the eighteenth century, the average number of baptisms more than doubled, and unless we assume that the total population grew equally fast—which would imply that the rate of growth in Furness was as high as in the rest of Lancashire, and very much above the national average —the figures would certainly suggest that there was a *relative* decline in mortality among the younger age groups in the population.

This point illustrates both the strength and weakness of the volume. For it could be argued that by casting his net so wide, Dr. Marshall has attempted too much. Those who are less interested in the detail of local history may wish that he had concentrated more on a selective and comparative analysis of some of the outstanding problems of general interest suggested by the history of the area, even at the expense of a fully comprehensive chronological treatment. But few will doubt the importance of the evidence provided by the local investigator in contributing to the solution of wider questions, and the Barrow Committee is to be congratulated on its enterprise

in making available a valuable addition to the stock of local studies which are helping to enrich our knowledge and understanding of the past.

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History of the Brewing Industry in England, 1700–1830. By P. MATHIAS. (London: Cambridge University Press, 1959. Pp. xxviii + 596. 85*s*.)

THIS is a work which deserves praise from every point of view—as a piece of sustained, critical research, as a valuable contribution to the study of the Industrial Revolution, as perhaps the first scientific investigation of the development of the brewing industry in this country. In this latter context its value is greatly enhanced by the fact that the author has had access to unpublished material and to the papers and records of some of the most famous brewing firms which tradition and caution have hitherto kept hidden —and it is to be suspected largely forgotten—in the companies' vaults. Even from this point alone the book marks a stage in the study of this industry in providing so much material for further use. How much more does the author achieve and where do we arrive? The book, for all its 600 pages, is a section only of the problem, a section in time and a section in place; that is to say about a hundred years in time, and virtually restricted to London in place. There are obvious reasons for this restricted approach, and it is in the pattern of modern historical research; it may indeed be argued that anything else is virtually impossible. But it has to be realised that such an approach must involve the great danger of overemphasis and of distorted perspective.

Very briefly the theme of the study is that " porter " was invented at the beginning of the eighteenth century; that porter was particularly and uniquely suitable for large-scale brewing; that the problem and cost of transport and the poor keeping qualities of ale or beer as then known restricted large-scale brewing to areas where a large market was immediately at hand; and that therefore London was the obvious place in which large-scale development could alone and did first take place. In due course, a century later, improvements in knowledge and technique made it possible to treat ale on the same lines as porter; and the revolution in transport in the 1840s changed the whole market position. In other words, in the author's views porter and porter alone made possible that expansion of large-scale brewing that started and developed in the eighteenth century. It is an interesting suggestion, but it is doubtful if it can be substantiated. That large-scale development and all that goes with it occurred is undoubted—but can one name porter as the cause rather than the subject?

It is here that we see clearly contrasted the advantages and the disadvantages of taking a limited period in such a survey. The author is between a dilemma of his own creation. The germs of expansion in the brewing industry were in fact already there and working; new markets were inevitably developing that not only justified but also required wholesale production; an ingenious brewer produced the new "porter," a more uniform and better product than much that was then being sold; and it caught the public taste. At once others followed, and porter became the rage. One can perhaps see the first stages of this same thing happening to-day in regard to lager beer.

This in no way minimises the value of the author's meticulous account of how large-scale brewing developed in the eighteenth century; the problem first of all of a seasonal industry with its subsidiary problems of the financing of heavy outgoings at one period in the cycle and the handling of heavy accumulations of cash at another period. The growing needs of an expanding industry, the over-optimism of individual brewers, the effects of normal trade fluctuations and of abnormal crises due to wars and scarcity; all would have brought in the problem of finance and the need of the banker. But in this case the position was different from other industries developing in this same period, in that the brewing industry provided surprising amounts of largely short-term cash for use elsewhere. It was this that above all produced that close link between banking and brewing that marked the latter half of the eighteenth century and the early years of the nineteenth.

Equally noteworthy, as the author emphasises, was the interdependence of brewing and two important agricultural activities—barley-growing and hop-growing. No one has indeed ever devised any other use for hops—apart from the old-fashioned sleep-producing pillows of our grandmothers; and brewing came in time—but not in the period of this study—to dictate to a large extent the types of barley to be grown, and to provide a most profitable and assured barley market. It is, however, only in this century that in fact the brewing industry has materially contributed to the scientific side of agriculture.

The author is wholly right in claiming brewing as an important item in the industrial revolution; but it is difficult to accept his view that it led the industrial revolution. It is certain and interesting that it was perhaps the most notable example of that revolution in the south of England, and in particular that it linked London with the Industrial Revolution in a way that has not been recognised. The position of London as the centre and heart of the new industry of brewing is indicated by the fact that, although brewing was spread all over the country, it was not till 1822 that the country brewers organised themselves as a representative body; and then it was due to the fact that London brewers were having an altogether disproportionate effect on legislation and administration that affected the whole country,

where there were rapidly developing markets of their own in the Midlands and the North.

The author gives some very interesting figures of the early growth of the tied-house system. The policy is seen to date from a much earlier period than the ordinary man in the street or the present-day critic realises. Barclay Perkins, for instance, had over 500 tied houses in 1830, the end of the period under survey, and the three biggest London brewers had then about 1,400 tied houses between them. This pattern was at this period probably largely limited to London, but even so, it would be difficult to think of any other industry in this or any country where this type of direct link between supplier and retailer had proceeded so far. What is interesting is that in all this period and indeed during the whole of the nineteenth century this " tie " went largely unnoticed and uncriticised; it is only in this generation that it has attracted so much notice, due no doubt to the fact that now something like 95% of ordinary public-houses are " tied." The author records the developments and gives the motives and reasons that influenced this policy of the big London brewers; he does not attempt, except perhaps by remote implication, to express any view on the soundness of the policy, nor does he debate it from the broader economic point of view. Neither aspect can be ignored.

The system of controlled outlets is curiously enough the only direction in which integration has played a major part in the brewing industry. The author devotes chapters to malting, barley growing and hop growing. In malting practice varied greatly, some brewers did a large part of their own malting, others did none; but over the whole industry malting remained very largely in independent hands, while barley growing has always remained almost wholly independent; and hop growing has been indulged in by only very few brewers.

Beer has always—at least in the last three centuries—played a part in national life that in two ways differentiated it from other industrial activities. In the first place, as the author points out, its contribution to the national exchequer has always been important and at times fundamental. In the mid-1730s the malt tax and the beer tax between them contributed a quarter of the total national revenue from taxation. In 1806 malt beer and hop excises brought in almost one-fifth of the total revenue of Great Britain drawn from taxation. Why beer and alcohol have always been a major objective and victim of the Revenue may be a matter of some wonder, but it is not irrelevant, as the author points out, that the brewing industry was found to " tolerate increments in the rate of excise more successfully than any other commodity." Beer still contributes very substantially to the national revenue and still fairly successfully survives the heavy taxation load put on it.

The other point the author rightly makes is that beer has always had a too conspicuous position from the social and moral point of view. In the

latter part of the period covered by this book beer had become the temperance reformers' most effective weapon, both in this country and in Ireland, in their efforts to stem the growth of gin drinking. Since then we have seen the reformers attack the stalking-horse itself; and whatever individual views may be, it has until comparatively recently been the apparent accepted public view that there must be strict, and ever stricter, control on the retailing of beer. This is not an aspect to which the author devotes direct consideration, but he points out the economic fact that the excise duty had in the period under review, as it has always had, an important and a disproportionate effect on the pricing policy of brewers and publicans. It has tended to obscure the competitive aspect and certainly to obstruct pricing policy.

All this but tends to illustrate what a complicated subject the history of brewing really is; this book, long as it is, has not perhaps been able to do more than set the stage for a detailed and critical consideration. When that is done it would seem to the reviewer that a clearer and a truer picture will be obtained by eliminating to a large extent from the account the ancillaries—the brewing materials, the incursion of individual brewers into politics and the like; and at the same time lengthening the period of review. There is really no argument for stopping at 1830, except the untenable argument that there was a recognisable line between the "Age of Porter" and the "Age of Ale." But while making that comment one must again say what a remarkable achievement this book is and what a valuable contribution it is to a hitherto largely unstudied subject.

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NOTES AND MEMORANDA

A CASE STUDY OF RESPONSE TO PRICE IN AN UNDER-DEVELOPED COUNTRY

I

THE extent of the response of agricultural producers in under-developed countries to changes in prices and price relationships bears on a wide range of issues of analytical and practical significance, including among the latter problems of public finance and of price and income stabilisation.

There are, however, well-known and serious difficulties in measuring the degree of responsiveness of producers to price changes. There are the familiar problems arising from the usual absence in the real world of anything resembling closely the *ceteris paribus* of the theoretical formulations of functional relationships in economics. There are further difficulties created by the time lags between changes in agricultural capacity and changes in output; and also by the effects of uncertainty about the permanence of absolute and relative price changes. The problems of testing a hypothesis or of measuring the strength of a functional relationship make it difficult to reach objective assessments, and rival hypotheses are likely to flourish side by side, often deriving from opposing policy preconceptions and sometimes giving rise to opposing policy prescriptions.

Though certainly not unknown, situations exhibiting sharply the response of producers to a change in an economic variable are infrequent, and have rarely been described. Situations deliberately induced by the contrivance of changes in the variable, that is, where, in effect, an hypothesis has been postulated and put to the practical test, are even more unusual. There is, therefore, some general interest in the policy of some of the statutory marketing authorities in Nigeria of raising the quality of certain agricultural produce by offering price incentives to peasant producers. This policy was explicitly premised on the hypothesis that peasant producers respond to price differences, and would appropriately adjust their behaviour as producers to the incentives provided by the authorities. The results of this experiment supported the hypothesis in that the average grade produced has risen greatly. The explicitly formulated hypothesis, the introduction of the measures and the consistent results closely resemble the methods of observation and experimentation in the natural sciences. This note describes the measures taken by the authorities, and the response of producers; thus, in effect it reports on the process and the results of a series of contrived experiments.

II

All major agricultural exports of Nigeria are handled by statutory export monopolies (marketing boards) established during and after the Second World War. Two of the marketing boards, the Nigeria Cocoa Marketing Board and the Nigeria Oil Palm Produce Marketing Board, prescribed wide differentials in the prices paid to producers of cocoa and palm oil for various grades of these products in order to encourage the production of higher grades.[1] These differentials in producer prices greatly exceeded the grade differentials in the markets in which the Boards sold the produce.[2] The producer prices and grade differentials were announced before the opening of each season, so that there was no price uncertainty; and the agents of the board had to buy all supplies offered to them at not less than the announced prices. The producer therefore knew the additional returns for improving his output by more careful tending of the trees, gathering of the produce and preparation for sale to the boards.

The purpose of the prescription of wide grade price differentials and the results of this policy are frequently referred to in the annual reports of the two boards. Here are two extracts from the annual reports of the Nigeria Cocoa Marketing Board:

> "It is quite certain that, given an adequate cash incentive and the necessary instruction, the Nigerian farmer is capable of producing well-fermented cocoa. That he has not always done so in the past is attributed largely to the fact that the premia offered were too low, for it should be remembered that the fermentation process involves a loss in weight in comparison with beans which are dried only and not fermented" (Nigeria Cocoa Marketing Board, *Second Annual Report*, Season 1948–49, p. 22).
>
> "That the Board's policy of offering a premium on first grade quality cocoa has resulted in an ever-increasing effort to produce good

[1] The Nigeria Cocoa Marketing Board was established in 1947 and the Nigeria Oil Palm Produce Marketing Board in 1949. They succeeded the West African Produce Control Board established in 1942, which also had a statutory export monopoly. In 1954 the boards were placed on a regional as distinct from a product basis. The regional boards, each of which handles several crops but only part of the total export of each crop, continued the policy of their predecessors in paying wide price differentials for different grades with the same result as experienced by their predecessors. For convenience of presentation we have confined the discussion to the period before the regionalisation of the boards in 1954. The tables in this paper cover the entire period of operation of the two national boards, spanning the interval between their predecessor, the West African Produce Control Board, and their successors, the various regional boards.

Measures to improve quality by the offer of price inducements were consistently applied only in respect of cocoa and palm oil.

[2] We are not concerned here with the merits of these policies, which are wasteful, since they encourage uneconomic expenditure of effort, discourage economic expenditure of effort and also waste output already produced. This is particularly evident in the refusal to buy so-called sub-standard produce even when this has a positive commercial value. We have analysed the economic implications of these policies in an article "The Economics of Marketing Reform," *Journal of Political Economy*, June 1954.

quality cocoa, is illustrated by the progressively increasing percentage of the total annual tonnage of cocoa marketed in the first grade . . ." (Nigeria Cocoa Marketing Board, *Fifth Annual Report*, Season 1951–52, p. 10).

The following is a quotation from an annual report of the Nigeria Oil Palm Produce Marketing Board:

> " Undoubtedly the major factor in the progressive improvement which has taken place in the quality of Nigerian palm oil over the past five years has been the Board's producer price policy which has offered considerable financial inducement by way of price differentials in favour of the production of the higher quality grades. Under this incentive, the fruit has been harvested quicker and more care has been taken in its preparation. Higher prices have encouraged the greater use of hand-pressing machines which tend not only to improve quality but also have a more favourable extraction rate than that obtained by traditional methods, and therefore increase production " (Nigeria Oil Palm Produce Marketing Board, *Sixth Annual Report*, 1954, p. 14).

III

The results are shown in Tables I and II, which refer to cocoa and palm oil respectively.

The high degree of response of producers to the price incentives clearly emerges from these tables. Thus, the proportion of Grade I cocoa in the purchases of the Cocoa Marketing Board increased from 47% in 1947–48 to 98% in 1953–54; and, after the purchases of the two lowest grades had dwindled to very small quantities, the Board discontinued their purchase. Indeed, the increase in the proportion of Grade I cocoa under the stimulus of the price incentive was greater than appears from this series, which begins with the year 1947–48. Until 1947, under the West African Produce Control Board, the premium of Grade I over Grade II cocoa was only £2·5 per ton, and in 1946–47 the proportion of Grade I cocoa only 23% of total purchases. It was a deliberate early decision of the Nigerian Cocoa Marketing Board to substitute three new grades for the previous Grade II and to widen the differential between the Grade I price and the prices of the two lowest grades to £5·5 and £15 respectively; the proportion of Grade I cocoa rose to 47% in the 1947–48 season.

From its first year, 1949, the Oil Palm Produce Marketing Board widened the differences in prices paid for the different grades of palm oil. The producer prices paid by the West African Produce Control Board in 1948 ranged from £32·25 per ton for Grade I to £26·25 for Grade V, a spread of £6 per ton. The new Board widened the spread to £16·5 in the first two years of its operations. This had little effect on the composition of the Board's purchases. The Board increased the differentials more markedly in 1951; this was also the first entire year in which the Board bought the

TABLE I

Nigeria Cocoa Marketing Board Producer Prices for Cocoa and Composition of Purchases by Grades, 1947–54

	Grade I.		Grade II.			Grade III.			Grade IV.			Total purchases, '000 tons.
	A £.	C %.	A £.	B £.	C %.	A £.	B £.	C %.	A £.	B £.	C %.	
1947–48	62·5	47·0	60·0	— 2·5	24·7	57·0	— 5·5	21·3	47·5	—15·0	7·0	75·0
1948–49	120·0	76·0	115·0	— 5·0	21·2	105·0	—15·0	1·8	90·0	—30·0	1·0	109·0
1949–50	100·0	89·4	95·0	— 5·0	10·5	75·0	—25·0	0·1				99·1
1950–51	120·0	95·1	110·0	—10·0	4·9							110·3
1951–52	170·0	96·0	155·0	—15·0	4·0	Grade no longer purchased			Grade no longer purchased			107·9
1952–53	170·0	95·0	155·0	—15·0	5·0							109·0
1953–54	170·0	98·2	155·0	—15·0	1·8							97·4

Notes

1. Abbreviations:
   - A. Producer price of grade, in £ per ton.
   - B. Difference between price of grade and price of Grade I, in £ per ton.
   - C. Purchases of grade as a percentage of total purchases made by the Board.
2. Grade differences: Grades were differentiated on the basis of the proportions of unfermented or insufficiently fermented beans and of defective beans.
3. The producer prices in this table refer to those paid for main-crop cocoa which accounted for over 90% of total supplies. The small quantity of light-crop cocoa, harvested later in the season, was subject to a discount of £5 per ton for all grades (somewhat smaller in 1947–48), until parity of treatment was introduced in 1953–54.

TABLE II

Nigeria Oil Palm Produce Marketing Board Producer Prices for Palm Oil and Composition of Purchases by Grades, 1949–54

	Special Grade.			Grade I.		Grade II.			Grade III.			Grade IV.			Grade V.			Total purchases, '000 tons
	A £.	B £.	C %.	A £.	C %.	A £.	B £.	C %.	A £.	B £.	C %.	A £.	B £.	C %.	A £.	B £.	C %.	
1949	No special price			42·75	66·4	37·13	− 5·62	14·5	33·0	− 9·75	12·7	29·63	−13·12	6·0	26·25	−16·5	0·4	161·5
1950	53·0	+10·25	0·2	42·75	61·3	37·13	− 5·62	17·8	33·0	− 9·75	14·4	29·63	−13·12	5·8	26·25	−16·5	0·5	158·7
1951	71·0	+16·0	6·3	55·0	70·8	43·0	−12·0	11·4	34·0	−21·0	7·5	30·0	−25·0	4·0	Grade no longer purchased			135·2
1952	80·0	+19·0	29·6	61·0	56·3	47·0	−14·0	7·4	35·0	−26·0	5·2	30·0	−31·0	1·5				178·5
1953	75·5	+17·5	50·4	58·0	38·0	45·0	−13·0	7·2	34·5	−23·5	4·4	Grade no longer purchased						211·6
1954	65·0	+15·0	60·8	50·0	29·9	38·0	−12·0	4·4	33·0	−17·0	4·9							205·0

Notes

1. Abbreviations:

   A. Producer price of grade, in £ per ton.
   B. Difference between price of grade and price of Grade I, in £ per ton.
   C. Purchases of grade as a percentage of total purchases made by the Board.

2. Grade differences: The original five grades were based on the free fatty acid (f.f.a.) content of the oil, ranging from 0 to 9% f.f.a. content for Grade I by equal steps to 36–45% f.f.a. content for Grade V. This classification was maintained, except that in 1952 the permissible range for Grade IV was narrowed to 27–33%. The Special Grade refers to " edible " (as distinct from the other " technical ") oil, with a low f.f.a. content (up to 4½%).
3. The table refers only to palm oil produced by smallholders, and not to plantation palm oil.

Special Grade of oil at a special premium price.[1] The extent of the improvement in the produce bought by the Board is apparent in the statistics. Thus in 1954 purchases of the Special Grade constituted as much as 60% of all purchases, while, as for cocoa, purchases of the two lowest grades were discontinued after they had fallen to small quantities.

Another experience of the Boards' operations, which cannot be presented in the same way, also shows clearly the awareness of producers of economic opportunities and their readiness and eagerness to take advantage of them. Whenever higher prices were announced or even generally anticipated for the following season, producers and intermediaries withheld supplies in the closing months of the previous season, while supplies were rushed forward when a reduction was announced or anticipated. This is reflected in statistics of monthly purchases by the Boards, and was sometimes mentioned in their annual reports.[2]

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WELFARE AND EXPANDED CHOICE— PROOF OF THE GENERAL CASE

MR. NEWMAN'S comment [3] upon my earlier article [4] includes a request for a proof of the optimality of free exchange in the many-person, many-commodity case. The chief aim of this note is to comply with Mr. Newman's request, after first giving a more general and rigorous statement of my welfare criterion than in my original article.

My original welfare criterion was conceived in terms of, and is entirely satisfactory for, straightforward situations of choice subject to a budget constraint. It was, however, pointed out by correspondents [5] that my criteria were not satisfactory for some situations (in particular, those involving non-connected sets of opportunities). The present statement is couched, I believe, in both the most exact and the most general form of which the proposition is capable of so being.

[1] Price differentials in favour of the Special Grade were introduced towards the end of the previous year, in which purchases of this grade constituted 0·2% of total purchases.

[2] The willingness of peasants to take risks on anticipated price changes is nothing new in West Africa. Early in this century it was well known that producers withheld supplies of oil nuts when stocks at the processing plants were known to be too low. Cf. Charles Wilson, *The History of Unilever* (1954), p. 182.

[3] Peter Newman, "Mr. Lancaster on Welfare and Choice," ECONOMIC JOURNAL, September 1959.

[4] "Welfare Propositions in Terms of Consistency and Expanded Choice," ECONOMIC JOURNAL, September 1958.

[5] Especially E. J. Mishan and T. Majumdar.

Let X be a set of points a in commodity space, and let X' be the set of all points a' such that $a' \leqslant a$.[1] Then to any individual who chooses point a in X the set Υ represents:

(i) an expanded choice if there exists in Υ a convex subset which includes both the originally chosen point $\bar{a}$ and at least one point not in X';

(ii) a contracted choice if $\bar{a}$ is not in Υ, but Υ is a subset of X'.

Expansion or contraction of choice is taken as the criterion of an increase or decrease in the individual's welfare, and the usual Paretian individualist criterion for an unambiguous increase in group welfare is adopted.

In order to show the optimality of free exchange in the general case, it is necessary to show, for N individuals and M commodities, that, commencing from an equilibrium reached with different individuals facing different prices, there exists at least one set of prices, the same for all individuals, for which some choices are expanded and none contracted.

Let p_{ij}^1, x_{ij}^1 be the prices faced, and quantities chosen, respectively, of the ith good by the jth individual, in a marketing situation in which the total quantities of the goods are fixed. Then we wish to show that we can always find some set of prices p_i^2, common to all individuals, such that

(1) $\sum_i p_i^2 x_{ij}^2 = \sum_i p_i^2 x_{ij}^1$ for all j (the budget constraints)

(2) $\left.\begin{array}{l} \sum_i p_{ij}^1 x_{ij}^2 \geqslant \sum_i p_{ij}^1 x_{ij}^1 \text{ for all } j \\ \sum_i p_{ij}^1 x_{ij}^2 > \sum_i p_{ij}^1 x_{ij}^1 \text{ for at least one } j \end{array}\right\}$ (the criterion for increased welfare)

(3) $\sum_j x_{ij}^2 = \sum_j x_{ij}^1$ (fixed total quantities for the goods)

for some set of quantities x_{ij}^2 not identical with the original set x_{ij}^1.

Writing $\Delta p_{ij} = p_i^2 - p_{ij}^1$, $\Delta x_{ij} = x_{ij}^2 - x_{ij}^1$, then (1) — (2), and (3) become, respectively

(4) $\sum \Delta p_{ij} \Delta x_{ij} \leqslant 0$, all j; $\sum_i \Delta p_{ij} \Delta x_{ij} < 0$ at least one j

(5) $\sum_j \Delta x_{ij} = 0$, all i; not all Δx_{ij} to be zero.

From (5), at least one of the Δx's must be positive and at least one negative. Provided not all the Δp's are of the one sign, we can always choose the Δx's so that we pair a non-zero Δx with a non-zero Δp of the opposite sign and satisfy (4). That is, so long as there is at least one commodity for which there are at least two different prices, we can always choose a price between the two which gives an expanded choice to at least one individual without

[1] That is, X' includes X plus any "empty" spaces either between two points in X, or between points in X and the co-ordinate axes. For the ordinary budget-line situation X, X' are identical.

contracting the choice of any other. Conversely, if the initial equilibrium has been reached with uniform prices there is no new set of prices which will enable expansion of one individual's choice without contraction of another's.

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SOME MARSHALLIAN CONCEPTS: A COMMENT

PROFESSOR MAXWELL has suggested [1] that I should have followed the approach of Professor Ragnar Frisch to the concept of the representative firm [2] in writing my 1954 paper.[3] It seemed to me, however, that Professor Frisch's paper was somewhat confused. Some of the objections to Professor Frisch's doctrine have already been pointed out by Professor Robertson.[4] While Professor Maxwell believes he has avoided some of these difficulties, his re-interpretation of Frisch appears to me to leave the theory of the representative firm in a rather unsatisfactory state.

(1) Professor Maxwell assumes that the firms in the industry selected for study are not in perfect competition.[5] But he also appears to assume, and without explanation, that there is a single supply price for the industry's output.[6] He thus ignores all the problems connected with the supply curve of an imperfectly competitive industry, problems with which my paper was designed to deal explicitly.

(2) Professor Maxwell seems to wish to define the representative firm as that firm which is at any time operating at the minimum point of its average cost curve (p. 693) [7] and equating marginal cost to price (p. 695). We are not told how this situation is to be made compatible with profit maximisation under imperfect competition.

(3) Professor Maxwell may, however, wish to abandon profit maximisation in favour of full cost pricing.[8] In that case it is not made clear why there would be a single price in the industry.[9]

(4) These problems are not, I think, insurmountable. My difficulty is

[1] J. A. Maxwell, "Some Marshallian Concepts, especially the Representative Firm," this JOURNAL, Vol. LXVIII (December 1958), pp. 691–8.

[2] Ragnar Frisch, "Alfred Marshall's Theory of Value," *Quarterly Journal of Economics*, November 1950.

[3] J. N. Wolfe, "The Representative Firm," this JOURNAL, Vol. LXIV, June 1954.

[4] Sir Dennis Robertson, *Economic Commentaries* (London: Staples Press, 1956), pp. 11–25.

[5] Maxwell, *op. cit.*, p. 694.

[6] Maxwell, *op. cit.*, p. 693.

[7] On p. 693 Maxwell says: "The rising firms will, for a time, be able to lower unit costs by enlarging their scale of plant until they reach an equilibrium point; they will then be representative, and no more internal economies will be open to them." The discussion on p. 696 appears to support and repeat this argument.

[8] This appears to be confirmed by the description on p. 694.

[9] Professor Maxwell nowhere states the assumption of a single price for the product of an industry explicitly. But it appears to be implied by all of his argument, and is almost stated in footnote 1 of p. 694.

that Professor Maxwell simply ignores them. A more serious complication arises, however, when we ask what Professor Maxwell wishes to do with the concept of the representative firm. The answer is by no means clear, but the quotations from Marshall cited on pp. 691–2 suggest that he wishes to argue that the costs of production of the representative firm will determine price. Unfortunately Professor Maxwell's basic assumption that there is a whole range of possible values for the minimum points of the cost curves of the various firms making up the industry [1] makes this impossible. If there are a large number of firms in the industry and a wide range of values of minimum cost points, there will presumably be a number of firms whose cost curves reach a minimum at any particular value selected. Out of this group there should be at least one firm which is producing at a minimum cost output. If all we know about the representative firm is that it is producing at an output which equates both marginal and average cost to price then there will be a different " representative firm " for each price we select. The representative firm is price-determined rather than price-determining.

(5) Professor Maxwell may not, however, have intended to use the costs of the representative firm as the determinant of price. It may have been his intention to use this device simply as a means of focusing attention on the existence of a range of costs. Such concentration would be more understandable if some use was to be made of the fact. Unfortunately, Professor Maxwell tells us nothing about the characteristics of this size distribution, or of the forces which determine it. One possibility which might bear examination is this: suppose that for any output, the costs of firms whose cost curves reach a minimum at that output, form a size distribution. We might formulate a hypothesis that the price which would prevail in the long run in this industry would bear some relation to this size distribution. It might, for example, tend to equal the mode, or the median, or some other measure of this distribution. Such an hypothesis would at least give us a starting-point, even if the causal forces behind it remained unknown.

(6) It seems to me a shortcoming of Professor Maxwell's model that it offers little explanation of what appears to be the chief behavioural characteristics with which Marshall endowed the representative firm—its tendency to grow with a growth in the demand for the products of the industry of which it composed a part, or with an increase in the price–cost ratio of those products. Professor Maxwell's observations on this question are very sketchy. He says (on p. 697) that: " In all cases the external economies which come to the representative firms in the original (fishing) industry, may, with a lag, open to them the chance of new internal economies." [2]

[1] See the discussion and diagram on pp. 694–5. On p. 691 Maxwell answers the question: " The representative firm is representative with respect to what? " by saying that " Confirmation of the view that unit cost is the key can be found in many statements in the Principles."

[2] This topic is discussed, somewhat darkly, on p. 697. Maxwell argues that an increase in demand for the products of an industry may open up new external economies if the supplying industries were not initially in equilibrium.

Professor Maxwell does not tell us why external economies should be translated into internal economies, but his reasoning may be as follows: the external economies take the form of the reduction of the price of some input to each firm in the industry. This will, in the short run, lower the costs of all the firms in the industry, but this lowering of costs must be described as an external economy. The lowering of the price of this input may, however, allow some modification of production plans, with some substitution of the input whose price has been lowered for other inputs. The reduction in costs which results from this reorganisation might conceivably be regarded as an internal economy. Depending on the technical characteristics of production, this reorganisation may increase, decrease or leave unaffected the output at which minimum cost is attained. If there were a single firm which reached minimum cost output at any price, and if price fell to exactly the same degree as the average cost of the firm that was operating at minimum cost before the change, this would specify the direction and extent of change in size of the representative firm in Professor Maxwell's model.

No doubt other interpretations of Professor Maxwell's argument are possible. I would myself prefer an entirely different approach, however, one which treats " the representative firm " as an abbreviated notation for the size distribution of firms, and which makes this size distribution the result of a process of chance. The analysis of the size distribution of firms, and with it price and output determination, would then centre on the study of the conditions which make the growth of firms more or less likely. Such a theory is hinted at in my 1954 article, and it is to be developed rigorously and in much greater detail in a forthcoming paper.

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INDUSTRIAL CAPITAL STOCKS AND ENERGY CONSUMPTION—A COMMENT

MR. FRANK'S article [1] in the March 1959 number of this JOURNAL may mislead readers into thinking that he has established more than he has, in fact, done. In his article he gives indices of capital stock and energy consumption, with the same year taken as 100, both for the United States and for the United Kingdom, and shows that the correlation coefficients between the indices, for each country, are very large indeed. But it does not follow from this that energy consumption may be used as an index of the stock of capital. A further examination of the statistics adduced shows that, in one of the three cases which he quotes, serious errors would result from doing so.

If, wise after the event, one wanted to use one of Mr. Frank's indices of

[1] A. G. Frank, " Industrial Capital Stocks and Energy Consumption," ECONOMIC JOURNAL, March 1959, pp. 170–4.

energy to estimate capital stock, the high correlation coefficients establish that, and only that, a linear function of an index of energy can be used as an index of capital. That is, writing x_t, z_t for capital and energy in the year or period t, a function of the form $\alpha + \beta z_t$ can be discovered which estimates x_t with considerable precision. It would not be correct to use z_t without further ado for this purpose; this assumes that $\alpha = 0$ and that $\beta = 1$. There are three pertinent questions: can α reasonably be taken to be 0, can β reasonably be taken to be 1 and would it be reasonable to take α as 0 *and* β as 1? Supposing that the discrepancies from the linear function of z_t are independent and normally distributed, these questions can be answered from Mr. Frank's figures.[1]

For the United States, a function of the required form, fitted by least squares, is

$$x_t = 1{\cdot}1298 + 0{\cdot}9873\, z_t, \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

and it appears *prima facie* that α and β might well have the values 0 and 1, the discrepancies being due to unrepresentative data. This is in fact so. With 95% confidence it may be asserted that, irrespective of β, α lies in the range $-7{\cdot}00$–$9{\cdot}26$. Irrespective of α, with the same confidence it may be asserted that β lies in the range $0{\cdot}95$–$1{\cdot}02$. Testing whether the real relation could be $x_t = z_t$ is a little more complex; it amounts to testing whether the point (0, 1) lies within a certain ellipse in the α, β plane. In fact, it does so in this case, so the observed statistics might well be represented by the function $x_t = z_t$. This confirms the impression produced by Mr. Frank's Fig. 1.

It is reasonable to ask what loss of precision would be involved by taking z_t as an estimate of x_t rather than using the better-fitting least-squares line (1). The answer is, very little. The greatest discrepancy between the two series of index numbers is 16, whereas the greatest discrepancy of the value estimated from (1) from the actual value is 14·46. The root mean square deviations are, respectively, 6·07 and 5·69. The improvement resulting from the use of (1) is slight, particularly compared with the inaccuracies of the data.

Using the Giffen and Stamp series of all capital, the regression for the United Kingdom is

$$x_t = 0{\cdot}4855 + 1{\cdot}0209\, z_t \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Again it turns out to be the case that the discrepancies from $x_t = z_t$ may reasonably be due to sampling errors alone. The confidence intervals for α and β, disregarding the other, are $-38{\cdot}82$–$39{\cdot}79$ and $0{\cdot}80$–$1{\cdot}24$ respectively (the comparative wideness of the intervals reflects both a smaller correlation coefficient and fewer observations). Once more, the point (0, 1)

[1] See, for example, A. M. Mood, *Introduction to the Theory of Statistics*, Ch. 13 (New York, 1950.)

is found to lie well within the appropriate ellipse in the α, β plane, so the observed statistics might well be represented by the function $x_t = z_t$.

In this case, the greatest discrepancy between the two series of index numbers is 14, whereas the greatest discrepancy of the value estimated from (2) from the actual value is 17·39. The root mean square deviations are 9·37 and 8·36 respectively. Again, (2) hardly represents an appreciable improvement.

The Douglas series of industrial capital for the United Kingdom gave Mr. Frank's lowest correlation, 0·9619. In this case, the results of the closer analysis are rather different. The regression is

$$x_t = -51{\cdot}7178 + 1{\cdot}4841\, z_t \; . \; . \; . \; . \; . \quad (3)$$

The confidence intervals for α and β, disregarding the other, are $-118{\cdot}75$–$15{\cdot}32$ and $1{\cdot}11$–$1{\cdot}86$ respectively. The former barely includes the value 0, indicating that this value is not implausible. But $\beta = 1$ is not a good approximation at all. Further, the ellipse in the α, β plane does not, this time, come anywhere near to including (0, 1) (in fact, $F_{(2,\,5)} = 50{\cdot}48$), so it is highly unlikely that $x_t = z_t$ could have led to the observed data.

The greatest discrepancy between the two series of index numbers is 64, whereas the greatest discrepancy from (3) is 27·81. The root mean square deviations are 41·81 and 14·26 respectively. It follows that (3) is a great improvement over $x_t = z_t$, as the statistical analysis indicates.

It is not possible to determine whether the radically different results of the two United Kingdom analyses are due to faults in the basic data or to a breakdown in the model. Mr. Frank argues that energy consumption should be a better indicator of industrial capital than of total capital, whereas this is the opposite conclusion to that reached from the figures. But an examination of (3) does indicate a possible explanation of the observed result. The regression indicates that a rise in energy consumption of one unit is associated with a rise in industrial capital of about one and a half units, where in each case the " unit " is 1% of the 1865 figure. This is in accordance with the argument that newer capital is a more efficient user of energy than older, at any rate in the industrial sector. The series for total capital, and for industrial capital in the United States, do not, however, reflect this.

The formal properties of the regression are as follows. In all cases the absolute rates of increase of the two series are in proportion and, in general, estimates of capital relating to two periods are needed to enable the series of energy to be translated into one of capital. If $\alpha = 0$, so that the regression is $x_t = \beta z_t$, the absolute rates are in proportion, as before, and in addition the percentage rates of increase are equal. One estimate of capital stock (which might well be 100 for a suitable base year) suffices to estimate the relationship. If $\beta = 1$ but α is not equal to zero, so that the regression is $x_t = \alpha + z_t$, the absolute rates of increase of the two series are equal, but the

percentage rates are not. Again, one estimate of capital stock suffices. If, as was the case in two of the three instances considered, the regression is effectively $x_t = z_t$, the energy series can be used immediately for the capital series. The high correlations indicate that a linear regression is a good model; before supposing that $x_t = z_t$ further investigations would be necessary in particular cases.

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A NOTE ON PROFIT SHARING IN MIDDLE EAST OIL

PROFESSOR PENROSE's closely reasoned article in the ECONOMIC JOURNAL,[1] though it does not support a commonly held view that the Middle East oil countries are exploited "because of the monopolistic organisation of the industry," takes the position that the oil companies possess "superior economic power . . . arising from their ability to inflict a disastrous economic loss on the producing country" and that this power "gives them a bargaining position which *holds down* the share of the profits the producing country can obtain in an oil agreement. Thus the producing countries can still complain of exploitation in the sense that their resources are being used to earn large profits which do not accrue to them." [2]

The post-war record in the Middle East clearly supports Mrs. Penrose in some cases, notably that of the serious Anglo-Iranian Oil Company–Iranian Government dispute of 1948–53. But other very significant parts of the record are not explicable in terms of her analysis. The purpose of this note is to call attention to such parts of the record, and particularly to the important cases in which the evidence strongly indicates that the Government and not the Company enjoys superior economic power, especially the power in given situations to increase substantially its share of company earnings.

The first case occurred during the period of the Iranian dispute, and, given interdependence in oil matters and the sellers' market at the time, was doubtless influenced, at least in part, by the course of that dispute. The case in question concerns the action of the Saudi Government in 1950. Though the Company (Aramco) fits Mrs. Penrose's model—in that it (1) was at that time the only company in the country, and (2) is owned by some of the biggest international oil companies—the Government was able to take *unilateral* action which more than *doubled* its share of net earnings from Saudi production. The Company was not happy about the Government's decision, but it acquiesced in it and recognised that the Saudi Government

[1] E. T. Penrose, "Profit Sharing Between Producing Countries and Oil Companies in the Middle East," Vol. LXIX, June 1959, pp. 238–54.

[2] P. 251 (italics added).

could not legitimately be denied formula parity with Venezuela, which had pioneered the 50–50 sharing arrangement over the preceeding seven years.[1]

More recent, and in some ways even stronger, evidence also indicates the superiority of the Government's position in important situations if not generally to-day. Beginning in 1958, for example, the discounting of oil in world markets reached such proportions that Middle East posted prices—which are used by the Governments of that area to compute the sharing base—led to a marked overstatement of the " net earnings " of the Companies. The result was that, despite the buyers' market, the Government share in important cases was further increased by some 20%, without as much as a sign of company reaction of the general type suggested by Mrs. Penrose's analysis.[2] Moreover, it is relevant to point out that at about the same time the Venezuelan Government unilaterally increased its share by decreeing a new tax formula which reduces the company share approximately a fifth.

The dynamics of the international oil industry, and especially the large Middle East component thereof, appear to be generating forces producing an increase in the secular trend of the Government's share. Though this is not the place to go into details, considerations along the lines of Ricardian rent analysis and the development of Arab nationalism within the general context of Soviet–United States political relations, rather than oligopoly analysis, would appear to loom large as explanatory factors.

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THE MARGINAL EFFICIENCY OF CAPITAL

MESSRS. PITCHFORD AND HAGGER [3] show that, for the Marginal Efficiency of an asset to be defined uniquely by the expression,

$$S = \sum_{t=1}^{t=m} \frac{Q_t}{(1+r)^t} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(A1)}$$

(where S is the current replacement cost of the asset, $Q_n \ldots Q_1$ are the

[1] See Aramco's testimony before Congress, *Joint Hearings before Subcommittees of the Committe on the Judiciary and Committee on Interior and Insular Affairs*, Senate, 1957, Part 2, pp. 1428–30.

[2] The adverse impact on Middle East oil companies was so heavy that one study even reports it in terms of a negative " indicated nonproducing income " in 1958 of $260·1 million in a year when adjusted company payments to producing governments amounted to $1,237·0 million, 18% greater than in the preceding year. See W. S. Evans, *Petroleum in the Eastern Hemisphere*, First National City Bank of New York, April 1959, pp. 12–13. Though this study includes areas outside the Middle East, the data in it may be used as Middle East figures.

[3] J. D. Pitchford and A. J. Hagger, " A Note on the Marginal Efficiency of Capital," ECONOMIC JOURNAL, September 1958, pp. 597–600. C. S. Soper, " The Marginal Efficiency of Capital: A Further Note," ECONOMIC JOURNAL, March 1959, pp. 174–7.

expected net returns during the periods of its life and r its marginal efficiency), it is necessary for

$$\sum_{t=1}^{t=m} t \cdot Q_t \cdot p^{t-1} > 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(A2)}$$

$\left(\text{where } p = \frac{1}{1+r}\right)$. If (A2) is not satisfied for all types of possible project it will not be possible to determine exactly the level of aggregate investment at a given rate of interest. It is concluded that the economic importance of this depends on how prevalent negative Q's are: "it is of considerable importance if negative Q's are at all common and can be largely ignored if, as may be the case, they are something of a rarity."

It may be noticed immediately that the existence of negative Q's will not necessarily cause (A2) to fail. It is the contention of this note that while Q's may often be negative, it is much rarer for them to be sufficiently negative for the marginal efficiency of the asset to be multi-valued.

The expectation of negative Q's by the investor is not rare; indeed, if periods of a sufficiently short length are taken, it will be unusual for some Q or other not to be negative. There are many cases where "follow-through" investment is necessary in the early years of a plant; and the initial investment itself is likely to be spread over a period of time. In other cases the necessity of very substantial repairs part way through the life of the plant can be foreseen with certainty. And ventures are frequently undertaken where there is a high probability that some year or other will produce substantial running losses—for example, in retaining a labour force through a recession or in carrying a mine through a wash-out.

Nevertheless, although an entrepreneur may face the prospect of some running losses when he is considering investing in a particular venture, he will, *if he is free to terminate the venture at any time during the physical life of the equipment*, contemplate carrying on through the period of losses only if he expects the losses to be succeeded by more than compensating profits. If, looking into the period of the physical life of the assets associated with a particular venture, a point is viewed beyond which losses over-balance profits, then this point is either the limit or beyond the limit of the economic life of the plant.

This proposition may be put in a much simpler way: from the streams of returns corresponding to the possible lengths of life the entrepreneur will select that with the greatest discounted value. Then if i is the rate of discount, $x = \frac{1}{1+i}$, the corresponding "discounting factor," and m the economic life of the plant, we have

$$\sum_{t=1}^{t=m} Q_t x^t \geqslant S \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(B1)}$$

which is the condition for the project to be economic; and the $m - 1$ conditions,

$$\sum_{t=1}^{t=m} Q_t x^t \geqslant \sum_{t=1}^{t=m-1} Q_t x^t \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(B2)}$$

$$\sum_{t=1}^{t=m} Q_t x^t \geqslant \sum_{t=1}^{t=m-2} Q_t x^t \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(B3)}$$

$$. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(B4 to B}\overline{m-1}\text{)}$$

$$\sum_{t=1}^{t=m} Q_t x^t \geqslant \sum_{t=1}^{t=1} Q_t x^t \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(B}m\text{)}$$

which are necessary conditions for m to be the economic life of the project.[1] (B2) to (Bm) can be rewritten in the forms,

$$\sum_{t=m}^{t=m} Q_t x^t \geqslant 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(C2)}$$

$$\sum_{t=m-1}^{t=m} Q_t x^t \geqslant 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(C3)}$$

$$. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(C4 to C}\overline{m-1}\text{)}$$

$$\sum_{t=z}^{t=m} Q_t x^t \geqslant 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(C}m\text{)}$$

In this form they restate the argument of the last paragraph. Adding (B1), (C2) to (Cm) we obtain,

$$\sum_{t=1}^{t=m} t \,.\, Q_t \,.\, x^t \geqslant S;$$

or, dividing both sides by x,

$$\sum_{t=1}^{t=m} t \,.\, Q_t x^{t-1} \geqslant \frac{S}{x} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(D)}$$

If (D) is true, (A2) is true and i is single-valued. And this will be true in the limiting case where $\sum_{t=i}^{t=m} Q_t x^t = S$ and $i = r$ or the marginal efficiency of the project.

Moreover, in proving (D) for cases of *costless extricability* we have proved (A2) with a margin to spare. It seems to follow that in some cases, even where the assumption of costless extricability fails, (A2) may still be satisfied. These are the cases where the unavoidable loss later in the life of the plant is small in relation to the initial capital outlay.

The Pitchford and Hagger case seems unlikely to occur, because it is surely true that costs of extrication are relatively small except where they are voluntarily incurred because of offsetting salvage values (*e.g.*, taking

[1] That the argument here is mathematically identical with that of Mr. Soper is evident from the fact that these are the conditions which he derives for the roots of the marginal-efficiency equation to be positive.

out the pillars of a mine). But the case cannot be proved impossible, and it might even come into its own if there was an increase in the extent of legal and social obligations placed on entrepreneurs to compensate their redundant labour forces and to restore the sites they abandon. If, for example, the Electricity Authority were compelled to restore perfectly the sites of obsolete nuclear power-stations it is possible that, because the "final" outlay would be of a size comparable with the initial outlay, it would react perversely to changes in the rate of interest.[1]

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COMPLEMENTARITY AND FUTURE TIME: A NOTE ON THE MARGINAL EFFICIENCY OF CAPITAL

IN a Note in the September 1958 issue of this JOURNAL Pitchford and Hagger [2] explained that the marginal efficiency of capital could not always be given an unambiguous value, for a given replacement cost of capital and a given series of yields, if some of the yields were negative. In March 1959 Soper [3] gave an elegant analysis of the conditions in which negative yields could in fact give multiple values; and went on to explain in economic terms why these conditions would not prevail under profit-maximising conditions.

Unfortunately the latter part of this proof is fallacious. It is a variant of a faulty theorem used in Hicks' *Value and Capital* to establish a different proposition.[4] Nevertheless, the derivation of the very limited conditions in which multiple values can occur certainly adds to our knowledge. When the technical conditions are set out in terms of actual economic possibilities they can be judged to be infrequent. The fallacy may, however, have some limited methodological importance.

Soper demonstrates that there cannot be more than one real and positive root to the equation $\sum_{i=1}^{n} Q_i p^i - S = 0$ (where Q_i is the output at time i

[1] What would happen in a case like this is that the revenue of the first few years of the station's economic life would pay for its initial cost, while the income of subsequent years would accumulate to provide for the "final" outlay. Thus for part of its life the authority would be a borrower and for part a lender, and if the latter was more important it would regard an increase in rates of interest favourably!

[2] J. D. Pitchford and A. J. Hagger, "A Note on the Marginal Efficiency of Capital," ECONOMIC JOURNAL, September 1958.

[3] C. S. Soper, "The Marginal Efficiency of Capital: A Further Note," ECONOMIC JOURNAL, March 1959.

[4] J. R. Hicks, *Value and Capital*, 2nd Edition, pp. 201, 221–2; T. H. Silcock, "A Note on a Theorem in Value and Capital," *Review of Economic Studies*, XVIII (2), 1950–51.

and $p = \frac{1}{1+r}$) when the value of S, the replacement cost, is continuously greater than $\sum_{i=1}^{i=k} Q_i p_j^i$ for all $k = 1, 2 \ldots n-1$ the term p_j being one real and positive root of the original equation. He then illustrates, by Fig. 1,

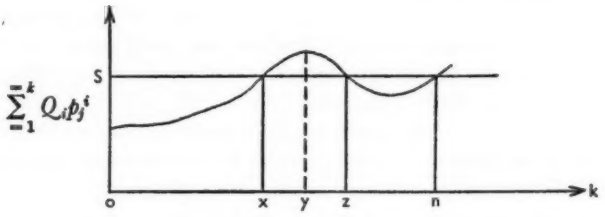

FIG. 1

the conditions in which there can be more than one real positive root to the equation. Multiple roots can only occur when the curve of the value $\sum_{i=1}^{i=k} Q_i p_j^i$ against time intersects more than once the horizontal line representing the replacement cost S.

From this he goes on to show that if the investment plan were discontinued at the point y the aggregate value, if discounted at the given rate p_j would be greater than the replacement cost. A smaller value of P, which would make the aggregate value equal to the replacement cost, implies a higher value of r, or in other words a higher yield. Thus, by discontinuing the investment plan at y the entrepreneur would secure a higher yield than in the original completed plan.

Since multiple values are only possible when the curve intersects the S line more than once, there must always be at least one point such as y at which, by stopping the investment plan, a higher yield could be secured than by carrying it through to completion.

In effect, this is a demonstration that there can only be more than one positive value for the marginal efficiency of capital in an investment plan, if there are not merely individual negative yields but whole sectors at the end of the plan, the addition of which reduces the aggregate discounted value below what it would be if the plan were discontinued earlier.[1]

The proof goes on to argue that, in the process of maximising profits, these sectors of the plan would be abandoned as unprofitable. A true maximum profit plan would therefore always have a uniquely definable marginal efficiency of capital.

[1] The passage in the text does not specify the rate at which the discounting must be done. It is in dealing with this nice logical point that Soper's proof is most elegant; but the matter does not lend itself to verbal exposition, and is not important for the present argument.

It is not, however, always possible to discontinue a plan at any point in time. Debts must be repaid, tin-tailings rendered harmless, building structures demolished and radio-active waste buried deep, at considerable cost. In making a plan in advance one may often have to face later, negative yields, as a condition of securing the earlier, positive ones.

In interpersonal relations an earlier consumption experience followed by a later outlay is fairly common. Most of us, who can get personal credit, have this experience every month! Is there any good reason for supposing that the pattern is uncommon in which the positive yield precedes some part of the negative yield which is its cost?

There is, of course, a difference in this respect between our dealings with nature and our dealings with other men. In our dealings with nature our plans consist in modifying, on the basis of more or less accurate knowledge, a part of the infinite series of events in an environment basically neutral or mildly hostile to our own ends. It is usually possible to ensure, at no great cost, that the series of events continuing after the planned satisfaction of our wants will be at least no more hostile to our own ends than if we had not intervened. If we know enough to make some events in the series markedly positive we shall normally know enough to keep the others neutral, ensuring that worn-out tools are not dangerous, and formerly cultivated land at least no more hostile or infertile than jungle.

This is the normal pattern, but unfortunately it is not invariable. Every plan we initiate now has an infinite series of positive and negative outputs as its consequences; and occasionally it is not possible, or not worth while, to take the positive action before the desired consequences, which will forestall the need to take remedial action afterwards, or suffer adverse effects.

Because of the normal pattern, however, our institutions are designed on the basis of physical property with a continuing positive value. Our economic morality is based on the assumption that it is wrong or irrational to enjoy something in advance of paying for it. Probably a number of technically feasible and economically rational possibilities of consuming in advance are ruled out on these grounds.[1]

More important is the fact that our normal conduct of economic life, and hence of economic analysis, is based on the assumption that bygones are bygones. Both obligations and assets surviving from the past are treated as institutional facts in the midst of which an essentially new decision has to be made. Investment, like all other economic decisions, begins anew at each instant.

From this point of view, if the entrepreneur has actually reached y, and if some of the negative yields beyond y are consequences imposed on him by his efforts and satisfactions up to y, then these negative yields will not, *at* y, be treated as part of his investment plan at all. They will be an institutional

[1] Cf. Joan Robinson, *The Accumulation of Capital*, Chapter 4.

hangover, unrelated to his future plans; and in planning for the future they will be ignored and treated as zero, once y is reached.

This is the reason why the fallacy in Soper's proof has some methodological importance. Indeed, this is the main significance of the present note. (Actual cases in which the latter end of an investment plan has negative value are unlikely to have much economic importance.) The significant point is that we cannot, when we are discussing economic time in mathematical symbols, treat it as if it were continuous without specifying our assumptions about the planning process much more carefully than we normally do.

Viewed from y, any negative yields which are merely a consequence of earlier investment are not part of the new investment plan at all. They have become simply a part of the environment; and to neglect this aspect of them would be to sacrifice the characteristic economic insights based on the essential newness of choice at each instant of time. But viewed from time 1, or the initial point of the whole investment plan, some of the negative yields may be technically complementary with some of the earlier positive ones.

Because it is easy and natural to treat time as a mathematical continuum, and because, in a continuing economic plan, the relations between anticipated time and realised time are complex, and are often the essence of the economic problem, it would appear to be desirable to translate mathematical reasoning as Soper did at this point, into verbal economic expressions, so as to facilitate intuitive checks on its plausibility.[1] If, in the proof under reference, the approach to maximisation by discarding any negative remainder of an investment plan after time y had been expressed in terms of a generalised mathematical statement, *e.g.*, that for at least some positive value P_j of P, the expression $\sum_{i=k}^{i=n} Q_i p_j^i$ must be positive for any value of k, and if this plausible condition, which would be valid for dealing with new investment but invalid for dealing with phases in an investment viewed from time 1, were then applied as a constraint in a general system of mathematical reasoning about the problem, it would be very difficult to detect that a flexibility rightly postulated at the initial point of an investment process had been wrongly assumed as between two future points of a present plan.

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[1] Cf. T. H. Silcock, "Mathematics in the Teaching of Economics," *Malayan Economic Review*, I, 1, 1956.

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DIFFERENTIAL POPULATION GROWTH AS A FACTOR IN INTERNATIONAL ECONOMIC DEVELOPMENT [1]

THE following note arises from Mr. P. T. Bauer's article on "International Economic Development." [2] In this article Mr. Bauer discusses Gunnar Myrdal's views on the divergence between, on the one hand, the increase in *per capita* national income shown, or assumed, in the great majority of countries *considered individually* and, on the other hand, the median income of the average world citizen. In this connection he refers to an article which I had published in 1949,[3] in which attention was drawn to this divergence. The reason for this divergence is, of course, the differential rate of population growth in the poorer and richer countries respectively. Since this matter is of some general interest, and there seem no real disagreements as to the points of substance—as distinct from the best way of expressing them and perhaps their quantitative importance—it may seem worth while setting out the points involved more clearly than has perhaps been done, and at the same time showing their quantitative importance in the light of recent figures.

The following table is based on U.N. data for *per capita* national income and growth of population respectively.[4] It shows countries grouped into four income classes, and the expected rate of growth of population 1955–65 for each class separately.

Income per annum *per capita*, $.	Number of countries.	Population 1955, millions.	Population 1965, millions.	Percentage population increase * 1955–65, %.
Under 100	51	1,437	1,713	19
100–599	73	835	1,001	20
600–999	9	199	211	6
1,000 and over	8	197	227	15
Total	141	2,668	3,152	18

* Data take account of international migration.

This table shows clearly that there is a generally negative correlation, although by no means a perfect one, between *per capita* income and rate of population increase. The rank correlation coefficient for 100 countries for

[1] The author, who at present is a staff member of the United Nations, is personally responsible for the opinions expressed, which do not represent the official views of the United Nations.

[2] ECONOMIC JOURNAL, March 1959.

[3] "Economic Progress in Underdeveloped Countries," *Social Research*, March 1949.

[4] I am indebted to my colleagues Mr. Grauman and Mr. Khan for the data included in this note.

which data were available individually is −0·33. (The Pearson coefficient of correlation is also −0·32, significant at the 1% level.)

Each of the two poorest categories of countries has a higher rate of population increase than either of the two wealthier categories. The quantitative importance of differential rates of population growth can also be readily calculated from this table. Other things being equal, *i.e.*, *per capita* income within each of the four categories of countries remaining constant,[1] differential population growth would reduce the world average income from about $341–342 in 1955 to about $333 in 1965. This is a decrease of just under 0·3% per annum.

The same result of a 3% decline in world *per capita* incomes as a result of differential population growth, other things being constant, can also be obtained by using the data to produce a simple "two sector model" in which the countries with a *per capita* income of less than $600 per annum/*per capita* form one group, and the countries with over $600 the other. In this simple model countries under $600 have six times the population of the others (1955 data). Applying to these two groups the expected rates of population growth 1955–65, we note that the average income of the two groups combined also falls by 3% within the decade.

This rough calculation is an under-statement of the downward pull exerted by the differential population growth in the poorer countries, since we have not made allowance, so far, for the differential growth of population *within* each income category. More detailed calculations relating to 100 countries suggests that the downward pull of differential population growth is 4·3% for the 1955–65 decade, or 0·4% per annum.

Now perhaps we may put the matter under debate as follows:

(1) To begin with, taking industrial output, agricultural output and other output separately there seems little observable tendency for output in the under-developed countries generally to grow less than output in the more developed countries generally. To repeat, that is the situation *as long as we consider the various sectors separately.*

(2) However, throughout the world there is a tendency for agricultural output to grow more slowly than industrial output. Since agricultural output is more heavily weighted in the national incomes of the poorer countries, this results in a general picture of national averages growing more slowly in the poorer countries, although there are numerous exceptions to this general tendency. Thus, the different weighting of the different sectors of output and its correlation with *per capita* income levels exerts a downward pull on the *relative* rate of progress of the under-developed countries, compared with others.

[1] For purposes of this rough calculation, $75 has been assumed to be the average *per capita* income in the lowest income class, of under $100 per annum *per capita*.

(3) Furthermore, and additionally to the downward pull mentioned under (2) above, there is differential population growth and its generally negative correlation with *per capita* incomes. This further downward pull reduces the world average income *per capita* at a present rate of 0·4% per annum. This means that if average *per capita* incomes *in the individual countries* increase by, say, 1¼% per annum—1% for the poorer countries and 2% for the other countries—the differential rate of population growth would offset, *for the world as a whole considered as a statistical unit*, about one-third of the progress suggested by the analysis of individual countries (0·4% out of 1¼%). Thus the offsetting factor is certainly significant, and it may well be that at times it has offset most or perhaps the whole of the progress observed in individual countries.

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GALBRAITH ON COUNTERVAILING POWER: A REPLY

THE difficulty in replying to Professor Galbraith's comment (March 1958) on my critique of countervailing power is to know his present position. Is his book on countervailing power intended as enlightened comment on the intellectual parochialism of professional economists, does it provide an interesting qualification of existing theory or is it an important theoretical innovation? I am puzzled to know at which level he is commenting.

On the first interpretation I do not feel competent to judge except to agree that the profession, all too frequently, seems to lose sight of the ends of our society in its preoccupation with the intricacies of analysis; or, worse, develops a mania for methodological problems. A sharp reminder of current realities from someone of Professor Galbraith's talents is always salutary. I myself confess to enjoying, and profiting from, his excursions into social and political-economy matters.

Secondly, if he means that countervailing power is a force to be reckoned with in market organisation and social and political life no one can possibly disagree. It may even be an important force. I myself adverted to its use, *à la* Galbraith, in legislation to control monopoly. It is now so incorporated into the 1956 Restrictive Trade Practices Act (Section 21 (*d*)). But it is not a new idea. Marshall, ever with us on these retrospective exercises, noted the possibility, indeed the probability, of a development of countervailing power in his time (*Principles*, 8th ed., p. 493). Nor is it easy to assess the place of such a notion in economics. Can it rank with what may be called the important secondary concepts of economic analysis—for

example, uncertainty, non-competing groups, increasing returns, the compensation principle, net advantages and other ideas associated with various names in economic thought? I would hesitate to attempt an answer.

However, in my innocence I took the third view that Galbraith was advocating, albeit on an intuitive level, a theoretical approach for market economies analytically superior to the orthodox competitive approach (see Chapters IV and IX of his *American Capitalism*). It was an attractive idea and seemed important. I therefore endeavoured systematically to test it by examining, not merely the structure of distribution as he implies, but also the interrelationships of manufacturing industry, the trading activities of government agencies, the habits of the co-operative movement and the development of the British trade-union movement (pp. 90–1, 93–5 and 95–8 this JOURNAL, March 1958). I found that competition was still the predominant force and that countervailing power made no systematic appearance in any sphere except, oddly, the buying activities of nationalised industries and government departments. The analytic deficiencies of competitive theory, of which Professor Galbraith makes so much, are irrelevant to this crucial point. Further, analysis revealed inconsistency in his selection of material and error in the principal welfare propositions of his theory (pp. 99–102). I see no reason now in the light of further thought, or his comment, to withdraw any of these conclusions. If Professor Galbraith proposes new theoretical constructions he must expect to have their operational value scrutinised; and for them to be shot at if found inadequate. This may be negative, but it is also an essential aspect in the advancement of economic thought—as Professor Galbraith must know.

ALEX HUNTER

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NOTE ON THE RELATION BETWEEN SHARE OF WAGES IN NATIONAL INCOME AND TERMS OF TRADE

"THERE seems to be some empirical support, therefore, for the view that movements in the terms of trade and in the share of wages are related (inversely), although it must be remembered that the number of countries we have considered is limited and the period with which we have been concerned short."

Thus Mr. Maynard [1] summarises his conclusions based on an analysis of eight countries over a limited number of post-war years.

[1] G. Maynard, "Inflation, the Terms of Trade and the Share of Wages in National Income," ECONOMIC JOURNAL, December 1958.

It is true that since 1939 there does appear to have been this inverse correlation; a regression line drawn on Chart 1 would proceed from north-

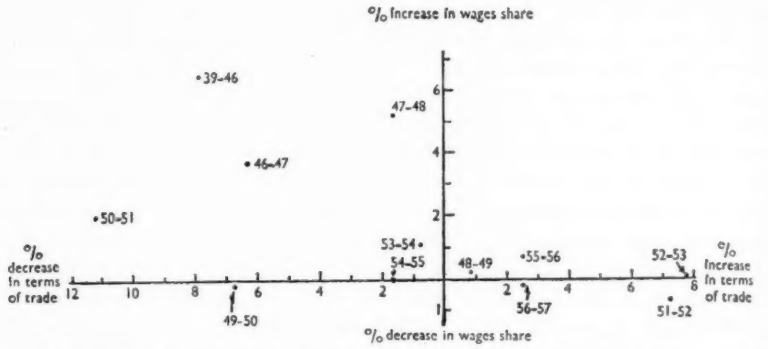

CHART 1.—Relation between proportionate change in wages share of national income and proportionate change in terms of trade (increase represents improvement), United Kingdom 1939–57.

west to south-east. There are, however, still three years when this negative correlation did not hold.

The scatter diagram in Chart 2 plots the relationship for seventy-one

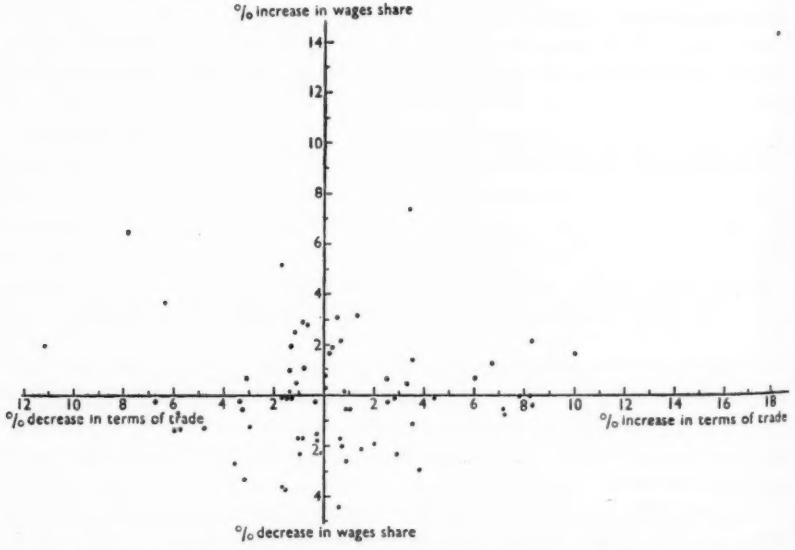

CHART 2.—Relation between proportionate change in wages share of national income and proportionate change in terms of trade (increase represents improvement), United Kingdom 1870–1957.

years or periods from 1870 to 1957 and it is obvious that there is no consistent long-run association between the two variables.

D. T. HEALEY

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Changes in the Share of Wages in the National Income and in Terms of Trade—United Kingdom 1870–1957

	% change in share of wages in national income.	% change in terms of trade.		% change in share of wages in national income.	% change in terms of trade.
1870–71	+1·3	+6·1	1906–07	+1·9	+0·3
1871–72	+7·4	+3·4	1907–08	+0·3	0·0
1872–73	+1·4	+3·5	1908–09	−1·3	−4·8
1873–74	−3·3	−3·2	1909–10	0·0	−1·4
1874–75	+2·9	−0·9	1910–11	0·0	+4·4
1875–76	−1·4	−6·0	1911–12	−2·1	−0·1
1876–77	−0·7	−5·9	1912–13	−1·1	+3·5
1877–78	−2·9	+3·8	1913–24	+14·5	+18·2
1878–79	+2·5	−1·2	1924–25	−0·2	−3·4
1879–80	−3·6	−1·7	1925–26	+0·5	+3·3
1880–81	−0·5	−3·3	1926–27	−1·7	−0·9
1881–82	+2·8	−0·7	1927–28	−3·6	−1·7
1882–83	−1·7	+0·6	1928–29	+0·8	0·0
1883–84	0·0	+2·8	1929–30	+2·2	+8·3
1884–85	−0·5	+0·9	1930–31	+1·7	+10·0
1885–86	−2·0	+0·7	1931–32	−0·5	+1·0
1886–87	+3·1	+0·5	1932–33	−1·9	+2·0
1887–88	0·0	−1·5	1933–34	+0·7	−3·1
1888–89	+3·2	+1·3	1934–35	−1·7	−1·0
1889–90	0·0	+8·2	1935–36	−1·2	−3·0
1890–91	+0·5	−1·1	1936–37	−1·3	−5·8
1891–92	+0·2	−1·4	1937–38	−0·3	+8·3
1892–93	+2·2	+0·7	1938–39	−2·3	−1·0
1893–94	−2·3	+2·9	1939–46	+6·5	−7·9
1894–95	−2·6	+0·9	1946–47	+3·7	−6·4
1895–96	+1·7	+0·2	1947–48	+5·2	−1·7
1896–97	−1·5	−0·3	1948–49	+0·2	+0·8
1897–98	−0·2	−0·4	1949–50	−0·2	−6·8
1898–99	−0·5	+7·1	1950–51	+2·0	−11·2
1899–1900	+0·7	+6·0	1951–52	−0·7	+7·2
1900–01	+1·0	−1·4	1952–53	0·0	+7·8
1901–02	−2·7	−3·6	1953–54	+1·1	−0·8
1902–03	+2·0	−1·4	1954–55	0·0	−1·7
1903–04	−4·4	+0·6	1955–56	+0·7	+2·5
1904–05	−1·8	−0·3	1956–57	−0·2	+2·5
1905–06	−2·1	+1·5			

Terms of Trade: The relationship between Import prices and Export prices; (a positive sign indicates an improvement in the terms of trade).

1870–1913 Imlah, "Terms of Trade of the United Kingdom 1798–1913," *Journal of Economic History*, November 1950.

1913–1957 *London and Cambridge Economic Bulletin.*

Sources: *Wage Share:*

1870–1945 E. H. Phelps Brown and P. E. Hart, "The Share of Wages in The National Income," ECONOMIC JOURNAL, June 1952.

1946–1957 Calculations from Table 2 of 1955 and 1958 Blue Books on National Income. Same definitions of National Income as used by Phelps Brown and Hart, *i.e.*, total personal income before tax minus pay of armed forces and employers' contributions. For comparison, the Phelps Brown and Hart figures appear below together with the figures calculated from the more recent sources:

Wages as % of National Income

	Phelps Brown and Hart.	New estimates.		Phelps Brown and Hart.	New estimates.
1938	39·2	39·3	1952	—	45·1
1946	40·5	40·8	1953	—	45·1
1947	40·8	42·3	1954	—	45·6
1948	41·7	44·5	1955	—	45·7
1949	42·3	44·6	1956	—	46·0
1950	41·9	44·5	1957	—	45·9
1951	—	45·4			

THE THEORY OF SELLING COSTS: A CORRECTION

In the paper of this title in the June issue of the Economic Journal I argued that the second order conditions of maximum profits require the return to selling outlay (at constant prices), to be diminishing. This argument is wrong. In correcting the proof I noticed that something was amiss and inserted a footnote (p. 296), in which diminishing returns to selling outlay became an assumption rather than a proposition. In the appendix, however, I allowed the mistake to stand.

If x is demand, $x_S = \frac{\delta x}{\delta S}$, $x_{SS} = \frac{\delta^2 x}{\delta S^2}$ (here S is selling outlay), then if the elasticity of demand is constant throughout, a second order condition for a maximum profit is $\left[\frac{x_{SS}}{x} - \left(1 + \frac{1}{e}\right)\right] < 0$, where e is the elasticity of demand. Hence $x_{SS} < 0$ is necessary only if $|e| \leq 1$.

The argument of the article is quite unaffected provided we treat $x_{SS} < 0$ as an *assumption*. If $x_{SS} > 0$ the equation (6*a*) of the appendix can be used to find the new shape of the *PS* curve which *may* now be positive, even if e is constant throughout. Since the case of a positively shaped *PS* curve is considered in the article (although I then believed it could only occur if e were not constant), the analysis can be corrected in an obvious way.

F. H. Hahn

Birmingham University.

CURRENT TOPICS

THE International Economic Association held a Conference at Elsinore, Denmark, during September 1959 on the subject of "Inflation." Among those who contributed papers were G. Haberler, H. S. Ellis, E. H. Chamberlin, J. S. Duesenberry, W. R. Gardner, A. W. Marget, R. Triffin (U.S.A.), J. C. R. Dow (U.K.), E. Lindahl, E. Lundberg (Sweden), E. James, J. Rueff (France), F. Neumark (Germany), P. Baffi (Italy), M. W. Holtrop (Netherlands), E. Gudin (Brazil). The papers and a summary of the discussion will be published for the International Economic Association by Messrs. Macmillan. The Conference was preceded by the triennial meeting of the Council of the Association. Professor Austin Robinson was elected to succeed Professor Lindahl as President for 1959–62; Professor Gudin was elected Vice-President; Professor Neumark as Treasurer.

Houblon-Norman Awards. The trustees of the Houblon-Norman Fund invite applications for Fellowships of Grants in aid of Research into the working of industry and finance in Great Britain and elsewhere and the economic conditions affecting them. Fellowships, which are intended for experienced research workers, are awarded for full-time research for one year, which may be renewed for a second year. Grants are awarded for either full-time or part-time work. The amount of the awards, which will take effect from October 1, 1960, will depend upon the circumstances of the candidate and the probable expenses of his work. Applicants must be British subjects normally resident in the United Kingdom. The Trustees are prepared to relax this rule only in exceptional circumstances. Forms of application, which may be obtained from the Secretary of the Fund, c/o the Bank of England, London, E.C.2, should be returned not later than March 1.

THE following have been admitted to Life Fellowship of the Society:

Agbi, B. O.	Kinoshita, Prof. K.	Muscatt, J.
Carney, D. E.	Machado, Dr. A. G.	Panas, E. G.
Katayama, S.		

THE following have been admitted to Fellowship of the Society:

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Ahmad, J.	Atkins, A. S.	Bennett, R. C.
Antony, T. J.	Aweh, F. O.	Britt Mother, E.
Arowolo, E. A.	Bailey, K. H.	Broughton, D. J.

Brown, G. R.
Butt, S. W.
Carneiro, D. A.
Chance, D. J.
Childs, A. E.
Clack, O. L.
Cohen, M. A.
Coker, C. H. H.
Collery, A.
David, C. A.
Davies, J. Rees
De Jong, H. W.
Dharchoudhury, N. R.
Dougan, K. A.
Elkan, W.
Enriquez, C. G.
Felicietti, A.
Ferguson, J. M.
Ghevaria, V. D.
Gogna, P. R.
Gordon, A.
Hall, C. A. Jnr.
Hambleton, H. G.
Harmon, Dr. E. M.
Higgins, V. E.
Hilmy, Dr. Y.
Hoare, D.
Hoare, G. E.
Hope, C. J.
Hotchkiss, W. E.
Houghton, R. W.
Hudson, M. W.
Hutton-Jamieson, N.
Johnston, J. S.
Jones, D.
Jones, O. V.
Jossa, Dr. B.
Kennerley, A.
Kenyon, C. G.
Khaliq, M. A.
Kim, J.
Kooraram, M.
Labsvirs, Dr. J.
Lamb, K. R.
Levi, L.
Lumb, D. M.
MacCallum, D.
Madan, J. N.
Manu, J. E. A.
McDonald, H.
McKenzie, J. C.
McSwiney, M.
Miller, S.
Miranda, R. A.
Mishan, E. J.
Mitter, D. K.
Mitter, P. B.
Monaghan, J. L.
Monaghan, P. A.
Monday, H. R.
Morgan, W.
Muir, I.
Nagpure, N. P.
Narain, D.
Nasir, Prof. M. S.
Newton, M. O.
Ojikutu, W. A. O.
Olaoshun, D. A.
Park, S.
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Shantharam Rao, N.
Sharma, Prof. H. S.
Sharma, K. P.
Shukla, Prof. D. K.
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Sinclair, W. A.
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Watson, D. J.
Wei Chi-Hua
West, Miss C. H.
Wijesinghe, S.
Williams, C. E.
Winrow, L. P.
Yonren, E. H.

Correction. See June Journal page 398. Kenzo, K. should be Kobayashi, K.

THE following have been admitted to Library Membership of the Society:

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Bhavan's College, Bombay, India.
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College of Commerce, Seoul University, Seoul, Korea.
Franklin & Marshall College, Lancaster, Pennsylvania, U.S.A.
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Institut für Agrarpolitik und Marktlehre, Kiel, Germany.
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Economica

VOL. XXVI, NO. 103, AUGUST 1959. *The Contribution of Economic Theory to Economic Prognostication:* R. C. TRESS. *Economic Growth and the Problem of Inflation: I:* N. KALDOR. *Advertising Appropriation Policy:* W. TAPLIN. *Ricardo on Factor Prices and Income Distribution in a Growing Economy:* H. BARKAI. *Stock and Flow Quantities: A Common Fallacy:* R. W. CLOWER; *The Rate of Interest and Walras' Law:* H. ROSE; *Reply to R. W. Clower and H. Rose:* D. PATINKIN. *"Coles": Weights and Measures, with Special Reference to Abraham Darby and the Coke-smelting Process:* R. A. MOTT.

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NEW BOOKS

British

ABRAMS (M.). The Teenage Consumer. LPE Papers No. 5. London: London Press Exchange (110 St. Martin's Lane, W.C.2), 1959. 9½". Pp. 25.

[This is an interesting and well-written short essay on the size of the teenage market and the special problems it poses for the manufacturer. Dr. Abrams estimates that the real earnings of teenagers have risen twice as fast as those of adults since 1938, and their "discretionary" spending (after deduction, of National Insurance contributions and payments to parents for board) has risen twice as fast as their earnings. Teenage spending is reckoned to comprise 6·3% of all consumers' spending, but its incidence is extremely uneven between commodities: it accounts for a quarter or more of total spending on bicycles and motor cycles, records, cinema admissions and cosmetics, but a negligible proportion of household durables, groceries and many others. The market is essentially a working-class one, because of the more prolonged education of middle-class boys and girls and their comparatively low starting salaries. This adds to the difficulties experienced by middle-aged, middle-class entrepreneurs in catering imaginatively for the market. An interesting contrast is drawn by Dr. Abrams between the powerful American influence on teenage consumption standards and habits and the remarkably conservative pattern of consumption that many working-class girls adopt when they become married and fall back on the guidance of their mothers.]

ALLEN (G. C.). British Industries and their Organisation. Fourth Edition. London: Longmans, 1959. 9". Pp. xi + 332. 25*s*.

[This very well-known book was thoroughly revised by its author in 1951, and has now again been largely rewritten. The historical approach, which has always been one of its most distinctive features, is retained, but the centre of attention is now shifted to the British economy of the late

1950s. Although the book is designed as an introductory one, Professor Allen contrives to include a considerable amount of detailed information on the industries he covers. A somewhat controversial element in his treatment of recent years is his attribution of many of the less satisfactory features of Britain's industrial performance to distortions caused by inflationary pressure.]

ASHTON (T. S.). Economic Fluctuations in England, 1700–1800. London: Oxford University Press, 1959. 9″. Pp. viii + 199. 21*s*.

[To be reviewed.]

BENHAM (F. C.). The National Income of Singapore 1956. A Chatham House Memorandum. London: Oxford University Press for the Royal Institute of International Affairs, 1959. 10″. Pp. 24. 2*s*. 6*d*.

[This is the first national-income estimate that has been made for Singapore alone. The method followed has been to estimate the consumption of private households and to add an estimate of net capital formation.]

BRENNAN (T.). Reshaping a City. Glasgow: House of Grant, 1959. 8½″. Pp. 221. 25*s*.

[To be reviewed.]

BROWN (E. H. PHELPS). The Growth of British Industrial Relations. A study from the standpoint of 1906–14. London: Macmillan, 1959. 9″. Pp. xxxvii + 414. 42*s*.

[To be reviewed.]

BUCHAN (A.). The Spare Chancellor. The life of Walter Bagehot. London: Chatto & Windus, 1959. 9″. Pp. 287. 25*s*.

[To be reviewed.]

Business Forecasting. Publications of the Market Research Society No. 3. London: The Market Research Society and The Oakwood Press, 1958. 8½″. Pp. 70. 15*s*.

[To be reviewed.]

CAMPS (M.). The European Free Trade Association. A preliminary appraisal. PEP Occasional Paper No. 4. London: Political and Economic Planning, 1959. 8″. Pp. 38. 4*s*.

[The author describes briefly the background of the negotiations among " the Seven " and then proceeds to an exposition and discussion of the Stockholm plan. In her view it is clear that although the plan as such brings certain advantages to the participating countries, the main motive behind it has been to improve the chances for an accommodation with " the Six." She recognises some merit in this argument, but she is apprehensive lest the formation of the Seven may in the long run serve to discourage the creation of closer links between the United Kingdom and France and Germany.]

CONDLIFFE (J. B.). New Zealand in the Making. A study in economic and social development. Second edition. London: Allen & Unwin, 1959. 9″. Pp. 316. 30*s*.

[To be reviewed.]

European Organisations. London: Political and Economic Planning (16 Queen Anne's Gate, S.W.1) and Allen & Unwin, 1959. 9″. Pp. xvi + 372. 30*s*.

[Successive chapters describe the structure and functioning of ECE, OEEC, the Council of Europe, NATO, WEU, ECSC, EEC, and Euratom. Much of the material is descriptive, but the book aims to be a work of interpretation and not merely a source for reference, and frank appraisals are included of the successes and failures of the various organisations. Particular attention is paid to the contrast between the two opposing concepts of the nature of authority in international organisations—the " inter-governmental " and the " supra-national."]

EVERSLEY (D. E. C.). Social Theories of Fertility and the Malthusian Debate. London: Oxford University Press, 1959. 9″. Pp. ix + 313. 35*s*.

[To be reviewed.]

FRIEDMANN (W. G.) (Ed.). Legal Aspects of Foreign Investment. London: Stevens, 1959. 9½″. Pp. xiii + 812. 147*s*.

[This volume contains surveys by different authors of the legal conditions affecting foreign investment in forty selected countries. These country surveys are followed by a brief comparative analysis by the editors and by an essay on the protection afforded to the investor by international law.]

HARBURY (C. D.). The Industrial Efficiency of Rural Labour. Cardiff: University of Wales Press, 1958. 8½″. Pp. ix + 228. 35*s*.

[To be reviewed.]

HINTON (R. W. K.). The Eastland Trade and the Common Weal in the Seventeenth Century. London: Cambridge University Press, 1959. 9″. Pp. xi + 244. 32*s*. 6*d*.

[To be reviewed.]

IRVINE (A. G.). The Balance of Payments of Rhodesia and Nyasaland, 1945–1954. London: Oxford University Press, 1959. 8½″. Pp. xiii + 643. 63*s*.

[To be reviewed.]

JOHNSON (L. G.). The Social Evolution of Industrial Britain. A study in the growth of our industrial society. Liverpool: Liverpool University Press, 1959. 9″. Pp. viii + 178. 25*s*.

[To be reviewed.]

KORNAI (J.). Overcentralization in Economic Administration. A critical analysis based on experience in Hungarian light industry. Translated from the Hungarian by J. KNAPP. London: Oxford University Press, 1959. 9″. Pp. xviii + 236. 32*s*. 6*d*.

[This is a translation of a book which originally appeared in Hungarian in 1957. The Hungarian edition was reviewed by Mr. A. E. Jasay in the ECONOMIC JOURNAL, December 1957, pp. 718–21. In a preface to the English edition the author refers to the reforms that have been made in economic administration in Hungary since the book was written. These have been in line with the proposals made in the book.]

LENNARD (R.). Rural England, 1086–1135. A study of social and agrarian conditions. London: Oxford University Press, 1959. 9″. Pp. 409. 45*s*.

[To be reviewed.]

MATTHEWS (R. C. O.). The Trade Cycle. Cambridge Economic Handbooks. Welwyn and London: Nisbet and the Cambridge University Press, 1959. 7½″. Pp. xv + 300. 12*s*. 6*d*.

[To be reviewed.]

Oxford Economic Atlas of the World. Prepared by the Economist Intelligence Unit and the Cartographic Department of the Clarendon Press. Second edition. London: Oxford University Press, 1959. 10½″. Pp. viii + 152. 42*s*.

[The *Oxford Economic Atlas* is in many respects more of a gazeteer than an atlas. More than half of its space is taken up by statistical tables and descriptive material, dealing, first, with world production and trade by commodities, and secondly, with the principal economic statistics of individual countries. Much of this material is naturally no more than a reproduction in very summary form of fuller data available without too much difficulty from other sources. The maps are all on a small scale. For these reasons the *Atlas* is likely to be less useful to many economists than the Oxford Regional Atlases, which appear in the same series. None the less, it is a convenient collection of up-to-date material.]

RICH (E. E.). The History of the Hudson's Bay Company, 1670–1870. Volume I: 1670–1763. Publications of the Hudson's Bay Record Society No. XXI. London: Hudson's Bay Record Society, 1958. 9½″. Pp. xvi + 687.

[To be reviewed.]

SCHWARTZ (G.). Bread and Circuses, 1945–1958. London: The Sunday Times, 1959. 8″. Pp. 220. 12*s*. 6*d*.

[This is a collection of the short articles that have appeared in Mr. Schwartz's celebrated column in the *Sunday Times*. Rereading them, one is conscious of the feelings they have always aroused: pleasure at his wit and forcefulness in driving home points with which one agrees, irritation at his seeming wrong-headedness in other matters. In the preface to the collection Mr. Schwartz states in more serious vein than usual the philosophy that underlies his approach.]

SMYTH (R. L.). The Distribution of Fruit and Vegetables. London: Duckworth, 1959. 9″. Pp. 232. 30*s*.

[This book covers much the same ground as the Runciman Report (1957); the conclusions are broadly similar, but they are based on more explicit analysis. There is much that is stimulating, but the descriptive sections are the more satisfactory (the chapter on the primary wholesaler is particularly good). When Mr. Smyth turns to analysis and appraisal of the performance of the fruit trade he accepts the virtues of competition yet is uneasy about the results. He sometimes hints at, but he does not come to grips with, the question as to whether there is too much competition and whether we pay too much for day-to-day equilibrium. The analysis is not sufficiently rigorous, and there are a number of apparently conflicting statements; nevertheless this is a useful book on a very difficult subject of considerable importance and much interest.]

STURMEY (S. G.). Income and Economic Welfare. London: Longmans, 1959. 9″. Pp. x + 208. 21*s*.

[To be reviewed.]

WILLAN (T. S.). Studies in Elizabethan Foreign Trade. Manchester: Manchester University Press, 1959. 9″. Pp. x + 349. 35*s*.

[To be reviewed.]

Australian

DOWSETT (W. T.). Elementary Mathematics in Economics. Nedlands, W. Australia: University of Western Australia Press (London: Pitman), 1959. 9″. Pp. xxi + 250. 30*s*.

[To be reviewed.]

New Zealand

WESTSTRATE (C.). Portrait of a Modern Mixed Economy: New Zealand. Wellington: New Zealand University Press (London: Cambridge University Press), 1959. 9″. Pp. x + 310. 22*s*. 6*d*.

[To be reviewed.]

Indian

GOPAL (M. H.). Towards a Realistic Tax Policy for India, 1959–66. The Presidential Address to the forty-first session of the Indian Economic Conference. Madras: University of Madras, 1959. 9″. Pp. 46.

[A detailed study of current Indian tax practice and of proposals that have been made to reform it (notably by Mr. Kaldor) leads the author to the conclusion that in the present circumstances of the country the objective of equity in taxation must take second place to the objective of stimulating growth, the two objectives being unhappily in conflict to a considerable extent.]

MOOKERJEE (SUBIMAL). Factor Endowments and International Trade. A statement and appraisal of the Heckscher–Ohlin theory. Published under the auspices of the Indian School of International Studies. Bombay: Asia Publishing House, 1958. 9″. Pp. viii + 99. 20*s*.

[To be reviewed.]

Ceylonese

LOGANATHAN (C.). Development Savings Banks. An institutional device to promote economic development and assist in the provision of social justice in Asian countries within the framework of democracy. Colombo: Ceylonese National Council, International Chamber of Commerce, 1959. 9″. Pp. xiii + 147.

[Under the scheme proposed here, a Bank would be set up which would receive compulsory and/or voluntary contributions from employers and employees, with various tax concessions to encourage voluntary contributions. It would also act as a channel through which part of aid granted by foreign governments to the government of Ceylon would be distributed to the economy. The funds at the disposal of the Bank would be invested in development projects on an equity basis,

in such a way as to enable small undertakings to participate to a greater extent than is usually possible from publicly raised finance while at the same time diffusing the profits over a wide class of the population.]

American

ADAMS (DORIS G.). Iraq's People and Resources. Berkeley, California: University of California Press (London: Cambridge University Press, 1959), 1958. 10.″ Pp. viii + 160.

[About half of this monograph is devoted to a discussion of population tendencies in Iraq and to a general cultural description of its people. The author then considers the level of income and consumption, and the potentialities of and obstacles to economic development. In many respects the purely economic problems of Iraq are less acute than in most under-developed countries. Oil resources form an ample source of finance, and this is made effective by legislation under which the greater part of oil revenues is awarded to the Development Board, a semi-autonomous body; at the same time the population is not excessive in relation to the amount of cultivable land, so that the application of most of the development funds to irrigation and other forms of agricultural improvements is a feasible policy. Cultural and administrative obstacles to development are none the less formidable. The chief value of Mrs. Adams's monograph is in her treatment of these problems, and in her description of the underlying social conditions, which is excellently done.]

AITKEN (H. G. J.) and others. The American Economic Impact on Canada. Durham, North Carolina: Duke University Press for the Duke University Commonwealth-Studies Center (London: Cambridge University Press), 1959. 9½″. Pp. xviii + 176.

[This contains the following papers: "The Changing Structure of the Canadian Economy, with Particular Reference to the Influence of the United States," by H. G. J. Aitken; "Recent American Influence in Canada, with Particular Reference to Economic Factors and Canadian Reaction," by J. J. Deutsch; "Canadian Economic Policy from 1945 to 1957—Origins and Influences," by W. A. Mackintosh; "The Impact of United States Farm Policy on Canadian Agriculture," by C. L. Barber; "The American Economic Impact on Quebec," by M. Lamontagne; "The Flow of United States Investment Funds into Canada since World War II," by I. Brecher.]

ALLEN (W. R.) and ALLEN (C. L.) (Eds.). Foreign Trade and Finance. Essays in international economic equilibrium and adjustment. New York and London: The Macmillan Company, 1959. 8½″. Pp. xii + 500. 42*s*.

[This is a collection of twenty-three reprinted articles, mostly very well-known ones. Most of them are reproduced in part only. Brief commentaries by the editors follow the five main sections into which the book is divided.]

BABSON (T. E.) and BABSON (D. L.). Investing for a Successful Future. How to surmount rising prices and high taxes. New York and London: The Macmillan Company, 1959. 8½″. Pp. viii + 312. 34*s*. 6*d*.

[The purpose of this book is to give advice to those who wish to plan their savings and the uses to which they are put on a long-term basis, with particular reference to provision for old age. In order to give the background to this, the authors survey some of the present characteristics of the American economy and explain why in their view conclusions drawn from the experience of earlier times, especially the crash of 1929, are likely to be misleading. They emphasise the need to form a clear plan and stick to it, acquiring securities with the desired characteristics and holding on to them without making any attempt to out-guess the market about short-term price movements.]

BACKMAN (J.). Wage Determination. An analysis of wage criteria. Princeton, New Jersey and London: Van Nostrand (358 Kensington High Street, London W.14), 1959. 9″. Pp. xv + 316. 51*s*.

[To be reviewed.]

BAIN (J. S.). Industrial Organization. New York: John Wiley (London: Chapman & Hall), 1959. 9½″. Pp. xviii + 643.

[To be reviewed.]

BAUMOL (W. J.). Business Behavior, Value and Growth. New York and London: The Macmillan Company, 1959. 8½″. Pp. xiv + 164. 33*s*.

[To be reviewed.]

BOCK (B.). Concentration Patterns in Manufacturing. Some findings from an inquiry into the relevance of data being used to measure market shares in specified industries. National Industrial Conference Board, Studies in Business Economics No. 65. New York: National Industrial Conference Board, 1959. 9″. Pp. 128.

[This is a factual survey, based on statistics published in a report from the Senate Subcommittee on Anti-Trust and Monopoly in 1957; these statistics were in turn drawn from data in the 1954 Census of Manufactures. The meaning of the statistics is examined, and a study is made of comparative degrees of concentration in different product classes and industries in 1947 and of trends in concentration between 1947 and 1954. The chief criteria used are the proportions of output due to the four largest companies and to the eight largest companies.]

BRADLEY (P. D.) (Ed.). The Public Stake in Union Power. Charlottesville, Virginia: University of Virginia Press, 1959. 9½″. Pp. x + 382.

[To be reviewed.]

BREMS (H.). Output, Employment, Capital and Growth. A quantitative analysis. New York: Harper & Brothers, 1959. 8½″. Pp. xiii + 349.

[To be reviewed.]

COLE (A. H.). Business Enterprise in its Social Setting. Cambridge, Mass.: Harvard University Press (London: Oxford University Press), 1959. 8½″. Pp. xiii + 286. 45*s*.

[To be reviewed.]

COMMONS (J. R.). Institutional Economics. Its place in political economy. In two volumes. Madison, Wisconsin: University of Wisconsin Press, 1959. 8″. Pp. xiii + 921.

[This is a reprint of one of Commons' principal works, which first appeared in 1934.]

Economic Lessons of Postwar Recessions. Report of the Committee on Economic Policy. Washington, D.C.: Chamber of Commerce of the United States, 1959. 8½″. Pp. 31. 50c.

[This contains a brisk survey of the course of events in America's three post-war recessions, with special reference to that of 1957–58, and of the opinions that have been expressed about them by economists and other commentators. The conclusions drawn are that complete stabilisation of the level of activity is bound to be extremely difficult and is perhaps not necessary so long as major depressions are avoided. In view of the difficulties of forecasting, substantial reliance on built-in stabilisers is recommended.]

FOX (H.) and WORTHY (N. B.). Severance Pay Patterns in Manufacturing. National Industrial Conference Board, Studies in Personnel Policy, No. 174. New York: National Industrial Conference Board (460 Park Avenue, New York 22), 1959. 11″. Pp. 48.

[This is a factual study of the extent and form of arrangements in existence in American manufacturing industry under which some compensation is paid to employees who lose their jobs because of factors outside their control, such as the shutdown of a plant or operation. The study is based on an inquiry conducted among some 700 firms. It emerges that rather more than half of the larger companies have some arrangement of this sort, and a lower, though still sizeable, proportion of smaller companies. The amount of the payment usually varies with the length of service; under the more generous schemes it may be of the order of a week's pay for each year of service, while under others there is a maximum of one month's pay.]

HARBISON (F.) and MYERS (C. A.). Management in the Industrial World. An international analysis. New York and London: McGraw-Hill, 1959. 9″. Pp. xiv + 413. 54*s*. 6*d*.

[The first part of this book is an essay of some 130 pages on the concept of management. This essay deals with management in turn as an economic resource, as a system of authority, and as a social class. Emphasis is placed on the mutual interaction of economic growth and changes in the nature of management. The second part of the book contains a series of papers by the two main authors and by a number of other contributors on management in the following countries; India, Egypt, Chile, Israel, France, Italy, Japan, Germany, Sweden, Great Britain, the Soviet Union and the United States. A final chapter deals with the management of foreign firms in underdeveloped countries.]

HAYNES (W.). Brimstone: The Stone that Burns. Princeton, New Jersey and London: Van Nostrand (358 Kensington High Street, London W.14), 1959. 8½". Pp. xx + 308. 45*s*.

[The first edition of this readable history and description of the sulphur industry appeared in 1942. It is not primarily an economic history, but the author has plenty to say about economic issues as well as about technical problems and about the personalities of the industry. Extensive statistics are provided in an appendix, and there are numerous photographs.]

HAZLITT (H.). The Failure of the "New Economics." An analysis of the Keynesian fallacies. Princeton, New Jersey and London: Van Nostrand (358 Kensington High Street, London W.14), 1959. 8½". Pp. xii + 458. 56*s*. 6*d*.

[The author has read assiduously Keynes' *General Theory* and the commentaries on it, and he works through the book pointing out all the fallacies he believes it to contain. Almost everything Keynes said is attacked with zest, not to say rancour. In such a cannonade it is inevitable that a certain number of targets should be hit; but the book cannot really be regarded as a serious contribution to knowledge. It is not, for example, at all on a par with that equally vigorous but much more discerning polemic, Terborgh's *Bogey of Economic Maturity*. The points that Mr. Hazlitt regards as irretrievable fallacies include many that have by now been settled to the satisfaction of almost all economists, such as the equality of savings and investment. Mr. Hazlitt's particular *bête noire* is Keynes' "anti-business bias," of which he finds traces on almost every page of the *General Theory*.]

HERSUM (A. C.). Operating Results of Variety Chains. Bureau of Business Research Bulletin No. 153. Boston, Mass.; Harvard University Graduate School of Business Administration (London: Bailey & Swinfen), 1958. 11". Pp. 37. 16*s*.

[This report is the 27th annual report in its series. Particulars are given of volume of sales, earnings, expenses and margins.]

HOROWITZ (M. A.). Commuting Patterns in Seven Standard Metropolitan Areas in Eastern Massachusetts, 1958. Boston, Mass.: Division of Employment Security of the Commonwealth of Massachusets (881 Commonwealth Avenue, Boston), 1959. 11". Pp. vi + 39.

[The findings are presented without much comment or attempt to draw conclusions of more than local interest. The main general conclusion appears to be that in a densely populated region like that studied, the labour market is a wide one and people are willing to travel long distances to work.]

KLEIN (B. H.). Germany's Economic Preparation for War. Cambridge, Mass.: Harvard University Press (London: Oxford University Press), 1959. 8½". Pp. xi + 272. 40*s*.

[To be reviewed.]

LEISERSON (M. W.). Wages and Economic Control in Norway, 1945–57. Cambridge, Mass.: Harvard University Press (London: Oxford University Press). 1959. 9½". Pp. xiv + 174. 36*s*.

[To be reviewed.]

LINDEN (F.). The Business of Department Stores. National Industrial Conference Board Technical Paper No. 7. New York: National Industrial Conference Board (460 Park Avenue, New York 22), 1959. 11". Pp. 48.

[This paper consists of two parts. The first reviews trends in department-store sales during the last ten years and considers to what extent the decline in the competitive position of department stores, which the statistics at first sight suggest, has in fact taken place. The second part deals with the effect of the weather on department-store business.]

MARCH (J. G.) and SIMON (H. A.). Organizations. New York: John Wiley for the Graduate School of Industrial Administration, Carnegie Institute of Technology (London: Chapman & Hall), 1959. 9½". Pp. xii + 262. 48*s*.

[To be reviewed.]

MARKHAM (J. W.). The Fertilizer Industry. Study of an imperfect market. Nashville, Tennessee: Vanderbilt University Press, 1958. 9½". Pp. 249. $6.00.

[To be reviewed.]

MASSON (R. L.). New Shares for Old. The Boston and Maine stock modification. Boston, Mass.: Harvard University Graduate School of Business Administration, 1958 (London: Bailey & Swinfen, 1959). 8½″. Pp. xvii + 398. 36*s*.

[This is a detailed study of the process by which the Boston and Maine Railroad made use of a 1948 amendment to the Interstate Commerce Act to simplify a complex and unwieldy hierarchy of stock classes and reduce the total structure to fit earnings prospects. The successive stages of the plan evolved by the management are described. The case-study is then used to cast light on the general issues involved in devising such a scheme and working out canons of fairness.]

PEN (J.). The Wage Rate under Collective Bargaining. Translated from the Dutch by T. S. PRESTON. Cambridge, Mass.: Harvard University Press (London: Oxford University Press), 1959. 9½″. Pp. xiv + 216. 48*s*.

[To be reviewed.]

RAPKIN (C.). The Real Estate Market in an Urban Renewal Area. New York: New York City Planning Commission, 1959. 9″. Pp. xii + 139.

[This is a study of the problems of a declining urban area, namely a group of twenty blocks in Manhattan's West Side. It is a heterogeneous district, including both expensive multi-storey apartment blocks facing Central Park and densely occupied brownstone rooming houses, let to an increasing extent to Puerto Ricans. The report contains voluminous statistics on types of owner and occupancy, prices and assessed values, mortgage availability and profits of owners. A paradoxical implication of the findings is that the social deterioration of a district does not necessary involve a decline in property values; indeed, the profits to be had from letting out a large old house on a one-room-per-family basis seem to be considerable. This means that the prospects of substantial demolition and redevelopment being carried out by unaided private enterprise are small, notwithstanding the attractive potentialities of the district, and public aid on a large scale is therefore likely to be needed to bring this about. The reader is left with slight misgivings about the economic rationale of a policy of large-scale demolition and replacement of existing property by dwellings designed for middle income groups, unless it is to be accompanied by an equally determined effort to provide attractive accommodation for the Puerto Ricans and others to whom these squalid and by no means cheap quarters are none the less at present the best available.]

RIPPY (J. F.). British Investments in Latin America 1822–1949. A case study in the operations of private enterprise in retarded regions. Minneapolis, Minnesota: University of Minnesota Press (London: Oxford University Press), 1959. 9½″. Pp. xii + 249. 40*s*.

[To be reviewed.]

SHUBIK (M.). Strategy and Market Structure. Competition, oligopoly and the theory of games. New York: John Wiley (London: Chapman & Hall), 1959. 9″. Pp. xviii + 387. 64*s*.

[To be reviewed.]

SILCOCK (T. H.). The Commonwealth Economy in Southeast Asia. Durham, North Carolina: Duke University Press for the Duke University Commonwealth-Studies Center (London: Cambridge University Press), 1959. 8½″. Pp. xvii + 259. 30*s*.

[To be reviewed.]

SMITH (E. L.). Common Stocks and Business Cycles. A practical analysis of the basic causes and patterns of cyclical behavior in economic series. New York: The William-Frederick Press, 1959. 9″. Pp. 226. $10.00.

[Mr. Smith has been concerned with stock-market problems for over fifty years, and he is the author of *Common Stocks as Long-term Investments*, which was published in 1924 and attracted a considerable amount of attention. In his new book he develops his ideas further in the light of subsequent experience, especially that of 1929–32. His theory of cyclical fluctuations is based on weather cycles, and a prominent part in it is played by the notion of 89½-year periodicity.]

Thirty Years of Research in Human Fertility: Retrospect and Prospect. Papers presented at the 1958 Annual Conference of the Milbank Memorial Fund. New York: Milbank Memorial Fund, 1959. 9″. Pp. 157. $1.00.

[The papers included in this symposium are divided into three groups, dealing respectively with Trends and Measures of Fertility, Fecundity and Contraception, and Social and Psychological Factors affecting Fertility. Most of the empirical material considered relates to the United States.]

TIMOSHENKO (V. P.) and SWERLING (B. C.). The World's Sugar. Progress and Policy. Stanford, California: Stanford University Press for the Stanford University Food Research Institute (London: Oxford University Press), 1957. 9½″. Pp. xii + 364. 52*s*.

[This appears in a Stanford series dealing with Food, Agriculture and the Second World War. However, it views the problems of sugar in a broader context than this might suggest, and though the effects of the war are fully treated, much attention is also paid to events since 1945. In the authors' opinion the International Sugar Agreement has not made any real contribution to the solution of the industry's main problems. These problems are largely political in origin, and stem from the disparity in the position of Cuba and other countries that sell the bulk of their output in the competitive world market, on the one hand, and colonial producers, such as Puerto Rico and the British West Indies, on the other, who have access to protected markets at above the world price.]

VICKERS (D.). Studies in the Theory of Money, 1690–1776. Philadelphia, Pennsylvania: Chilton Company, 1959. 8½″. Pp. viii + 313. $6.50.

[To be reviewed.]

WALD (H. P.). Taxation of Agricultural Land in Underdeveloped Economies. A survey and guide to policy. Cambridge, Mass.: Harvard University Press (London: Oxford University Press), 1959. 8½″. Pp. xiii + 231. 36*s*.

[To be reviewed.]

WARNTZ (W.). Toward a Geography of Price. A study in geo-econometrics. Philadelphia, Pennsylvania: Pennsylvania University Press (London: Oxford University Press), 1959. 8½″. Pp. 117. 40*s*.

[To be reviewed.]

WATILLON (L.). The Knights of Labor in Belgium. Translated by F. MEYERS. Los Angeles, California: Institute of Industrial Relations, University of California, 1959. 9″. Pp. 35. $1.50.

[This is a translation by an American labour historian of a pamphlet written in the 1920s. The author is now Director General of Social Security in the Belgian Ministry of Labour and Social Security. The pamphlet describes how organised labour in certain Belgian industries, especially glass-making and coal-mining, became affiliated to the American Knights of Labour. Whereas a similar affiliation in some British industries proved short-lived, the Belgian Knights of Labour remained in existence until the 1930s.]

ZANK (E. E.) and BAKKEN (H. H.). Light and Power. Rates and costs of service in Wisconsin R.E.A. Co-operatives. Madison, Wisconsin: University of Wisconsin Press, 1959. 9¼″. Pp. xii + 180. $4.00.

[Rural Electrification Administrative co-operatives were first set up under federal auspices as a relief measure in 1935. They were designed to bring electricity to regions considered sub-marginal by privately operated undertakings. They have continued to grow, though at a diminishing rate, as population and industry have spread into rural suburban areas. This study is intended primarily for those actually engaged in a managerial capacity in the co-operatives. It analyses the forms taken by operating costs, depreciation and other costs, and the relation of these to rate-fixing. The authors favour the adoption of a simple two-part rate in place of the more complex systems now in use.]

Dutch

ARROW (K. J.) and HOFFENBERG (M.). A Time Series Analysis of Interindustry Demands. Contributions to Economic Analysis, XVII. Amsterdam: North-Holland Publishing Company, 1959. 9″. Pp. 292. 55*s*.

[To be reviewed.]

BJERVE (P. J.). Planning in Norway, 1947–56. Contributions to Economic Analysis, No. XVI. Amsterdam: North-Holland Publishing Company, 1959. 9″. Pp. xx + 383. 70*s*.

[The author is Director of the Central Bureau of Statistics of Norway. His book is a critical appraisal of the national budgeting policy that has been pursued in Norway. This policy involves the annual publication of projections of the chief economic variables of the system, based on a comparatively unsophisticated theoretical model but involving a fair amount of detail. It has not always been quite clear whether these projections have the status of forecasts or of goals, but to some extent at least the original intention that they should be goals has been retained. The author's

general verdict on the system is favourable. He argues that the not infrequent deviations between projected and actual figures do not necessarily point to a fault in the system, and he argues that the need to produce consistent sectoral projections imparts realism to government policy, even if the principles of calculation are crude. Some faults have admittedly been present, particularly the insufficient attention given to prices in the budgeting system, and inflation has not been avoided. A curious feature has been that investment has persistently tended to absorb a larger proportion of gross national product than it was planned to do. Dr. Bjerve has written a clear and able study of an interesting experiment.]

HARTOG (F.). European Trade Cycle Policy. Leyden: Sythoff, 1959. 9½". Pp. 55. D.fl. 2.90.

[This pamphlet was written in 1956, but the author believes that subsequent events have served to strengthen, not weaken, his conclusions. He argues that the high-level stability enjoyed since the War is not something that can be permanently taken for granted. Policy to ensure it is needed, not least in the inflationary stage of the cycle, and this policy to be fully effective calls for a large degree of international co-operation.]

Swedish

Prospects of Development in Western Europe, 1955–1975. Industrial Institute for Economic and Social Research, Stencil 1. Stockholm: Industrial Institute for Economic and Social Research, 1959. 11½". Pp. 65.

[The author of this short study is Professor I. Svennilson. It contains statistical projections of output and employment by sectors for Europe as a whole and for groups of countries within Europe. The projections are in very broad outline, but the author gives a good lucid statement of the main considerations to be borne in mind in putting numerical values on such key variables as the saving-income ratio, the rate of growth of productivity and the rates of growth of the labour force in different sectors.]

Danish

ULDALL-HANSEN (H.). Tid og Rente. (Time and Interest.) Copenhagen: Dansk Videnskabs Forlag, 1959. 10". Pp. 248. Danish kroner 35.

[As described in the brief English summary, this is a theoretical and in parts mathematical analysis of the relationship between the length of life of a bond and its yield. An important part in the argument is played by the assumption that the future can be usefully divided into two parts, the " predictable " period, during which an investor may reasonably predict a trend in the interest rate, and the subsequent period.]

Finnish

HONKO (J.). Yrityksen Vuositulos. (The Annual Income of an Enterprise and its Determination.) Helsinki: Liiketaloustieteellinen Tutkimuslaitos, 1959. 10". Pp. 246.

[This is a theoretical work, which builds on the ideas of Fisher, Lindahl and others, in an attempt to distinguish the various meanings of the concept of income and their application. Emphasis is placed on the appropriateness of different concepts of income for different purposes. The author argues that a concept of income based on expectation would be superior to one based on realisation in a world of perfect foresight where income statements could be made as often as desired instead of at set annual intervals, but that in practice the concept based on realisation has to be preferred.]

LARNA (K.). The Money Supply, Money Flows and Domestic Product in Finland, 1910–1956. Helsinki: The Finnish Economic Association, 1959. 10". Pp. 227. Finnish Markkas 1000.

[The author considers first the concepts of money supply, money flows and marketed domestic product and their measurement for the Finnish economy. After a discussion of the decomposition of the time-series involved, with particular reference to seasonal fluctuations, he proceeds to an analysis of their mutual inter-relations. This is done first by working out the movement over time of the velocity of circulation, calculated in various ways, and then by a more elaborate econometric analysis. The purpose of the latter is to " explain " movements in marketed domestic product in terms of money supply and money-flow variables. The results are encouraging, and it is suggested that this approach may provide a useful method of computing an approximation to national income more rapidly than hitherto and for shorter periods than a year.]

Japanese

TAKAHASHI (C.). Dynamic Changes of Income and its Distribution in Japan. Institute of Economic Research, Hitotsubashi University, Economic Research Series No. 3. Tokyo: Kinokuniya Bookstore, 1959. 8½″. Pp. 182.

[To be reviewed.]

TAKENAKA (T.). Business and Government. Tokyo: Science Council of Japan, 1958. 8½″. Pp. 52.

[The author is interested in the currents of opinion which have shaped relationships between persons and government in the sphere of public utilities. He discusses this with special reference to the experience of the states of Wisconsin and California.]

Chilean

Outlook for Chile's Foreign Trade and Economic Growth, 1959–1965. Santiago: Institute of Economic Research, University of Chile, 1959. 10½″. Pp. 90.

[The greater part of this report is devoted to projection of Chile's capacity to import. It is estimated that the volume of imports should be able to rise at something near to the average rate recorded in recent years, namely somewhat less than 6% per year. The projections of gross national product are based on those of the capacity to import, and are likewise moderately optimistic. The analysis is confessedly incomplete, since no direct attention is paid to the problem of inflation, which has in the past beset the Chilean economy so severely. From 1953 to 1958 the real national income fell, while population increased rapidly. This was partly due to a decline in the capacity to import in 1954–55, but this factor was not in operation in 1956–57, and the stagnation of those two years may be attributed instead to the government's severe anti-inflationary programme. The authors' method of analysis implicitly assumes that growth in 1959–65 will not be subject to any obstacles of this sort.]

Official

BRITISH

MINISTRY OF AGRICULTURE, FISHERIES AND FOOD. Domestic Food Consumption and Expenditure, 1957. Annual Report of the National Food Survey Committee. London: H.M. Stationery Office, 1959. 9½″. Pp. vi + 137. 8*s.* 6*d.*

[This year's report follows the now familiar lines, with a number of additions. A chapter has been included on dietary differences associated with occupational status and skill. A special section compares the diets of families containing older and younger children.]

INTERNATIONAL

GENERAL AGREEMENT ON TARIFFS AND TRADE. International Trade 1957–58. Geneva: GATT, 1959. 9½″. Pp. 318. $2.00.

[Hitherto this publication has appeared annually; the present issue for the first time deals with developments over a period of two years. Much useful statistical information and comment is provided on trends in world trade in total, by commodity and by country. Particulars are also given of developments in the sphere of commercial policy.]

INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT. Fourteenth Annual Report, 1958–1959. Washington, D.C.: International Bank for Reconstruction and Development, 1959. 11″. Pp. 56.

[This year's activities are described, with details of the loans granted.]

INTERNATIONAL LABOUR OFFICE. Labour Costs in European Industry. Geneva: International Labour Office, 1959. 9½″. Pp. viii + 170. 7*s.* 6*d.*

[This study provides data on labour costs in selected industries in different European countries. Comparison is then made both between labour costs in different industries in the same country and between labour costs in the same industry in different countries. Particular attention is paid to the relative magnitude of basic wages, wage premiums (holiday pay, overtime premiums, etc.) and other labour costs (bonuses, social security contributions, etc.). The importance of this last item in total labour costs varies from 12% in the United Kingdom to 42% in Italy and 46% in Yugoslavia.]

INTERNATIONAL MONETARY FUND. Annual Report of the Executive Directors for the Fiscal Year ended April 30, 1959. Washington, D.C.: International Monetary Fund, 1959. 9″. Pp. 237.

[This report is devoted mainly to a survey of developments during the year in the spheres of international payments and domestic financial policies. Brief particulars of the Fund's own activities are also included.]

ORGANISATION FOR EUROPEAN ECONOMIC CO-OPERATION. The Work of the Organisation for European Economic Co-operation. A report by the Secretary-General. Paris: O.E.E.C., 1959. 9½″. Pp. 114. 7*s*.

[This is a general survey of the functions of O.E.E.C. It is designed to help create a better informed public opinion on the Organisation, and to place in their context the particular activities which from time to time attract unusual publicity.]

UNITED NATIONS FOOD AND AGRICULTURE ORGANISATION. FAO Mediterranean Development Project. The integrated development of Mediterranean agriculture and forestry in relation to economic growth. A study and proposals for action. Rome: F.A.O., 1959. 11″. Pp. 213. $2.00.

[Although much of this report is occupied by fairly technical discussion of the agriculture and forestry of the region, it has a more general interest as well, and care is taken throughout to view agriculture in the broader context of economic development as a whole. The first two chapters describe the general characteristics of the region and consider the objectives of policy and the roles to be played by different sectors of the economy and by the state. Chapters 3 and 4 then treat in more detail the programmes needed for increasing production and improving land use. This is followed by studies of individual countries, namely Iraq, Syria, Lebanon, Tunisia, Morocco, Spain, Yugoslavia, Greece, Israel and Turkey. The F.A.O.'s chief consultant for the project was Dr. Thomas Balogh.]

UNITED NATIONS FOOD AND AGRICULTURE ORGANISATION. The State of Food and Agriculture 1959. Rome: F.A.O., 1959. 11″. Pp. 197. 10*s*.

[This contains a survey of recent events and outlook and also two more general chapters. The first of these is devoted to the relationship between agricultural incomes and the level of economic development; the second considers certain problems of agricultural development in backward countries with special reference to marketing, land tenure and investment.]

UNITED NATIONS FOOD AND AGRICULTURE ORGANISATION. Yearbook of Fishery Statistics. Vol. VIII, 1957. Rome: F.A.O., 1959. 12″. Pp. xvi + 334. 20*s*.

[The last issue of this yearbook to deal with international trade in fishery products appeared in 1955. The present issue is differently arranged. It contains no separate country tables, but is divided up instead according to seven groups of fishery commodities. Each section shows imports and exports by country for the commodity group in question.]